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THE PSYCHOLOGICAL DEVELOPMENT OF VINCENT VAN GOGH

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VINCENT VAN GOGH was born at Zundert (Province of Noord Brabant) in 1853, the eldest son of a minister of the Protestant church. We know little of his childhood years; he is described as a rather headstrong boy, but not abnormally so, his sister (1) pictures him as a great nature-lover and rather an introvert. His mother, a sensible and strongminded woman, handled him with indulgence, his father, by nature a kindly man, could be stern and forbidding, yet all his children including Vincent adored him. He loved his brother, Theo, quite as much, "who had inherited his father's name and his heart's goodness".

From his 12th until his 16th year he was at a boarding school at Zevenbergen. At this time he was quite a normal boy, not markedly different in any way from the others, as the director of the school stated later.

We know little of his artistic abilities in his youth; probably there were no signs of any exceptional talent in this direction. From 1869 until 1873 he worked as a pupil and assistant in the art-dealer shop of Goupil in the Hague; his uncle, himself an art dealer, introduced him there. In the beginning he appeared to be perfectly adapted to the life and work and was diligent, courteous and kind; customers and painters equally liked him. After four years he went to the London branch of the firm with an excellent testimonial. It seems to me that most psychiatrists who have studied Van Gogh's life do not emphasize sufficiently that the youth Vincent was a perfectly normal, well-adapted and sociable young man, in sharp contrast to his bizarre and abnormal personality in later years. In London the extravert side of his character became even more marked. Here also he did his work with enthusiasm and devotion, was agreeable and sociable, and—imagine this in relation to the later Vincent!—went daily to the city elegantly turned out in a grey top-hat! Also he was pleased when the firm made good profits. We get a definite impression of a well-adapted young man, whose career, until his 21st year, is developing smoothly and easily; in this phase it is hard to find any trace of a psychopathic personality.

In London he soon falls in love with his landlady's daughter. At first this stimulates his "lust for life" even more. He writes home: "I have a wonderful home here" and in the letters he exults about "this rich life—your gift, O God!" His overflowing vitality also expresses itself in making drawings in the evenings along the Thames.

Then, about a year after his coming to London, a fundamental change occurs. He proposes to the girl, but learns that she is already engaged and therefore rejects his proposal. He cannot swallow this disillusion which proves to be a definite turning-point in his life. When he returns home on his holiday, his family finds him "changed", depressed, and extremely religious. Even more striking is his different attitude towards his work and his job. He is uncivil towards his customers, careless in his work and queer. When he is transferred to the firm's branch in Paris, it makes no difference; here he gets even more unsociable and queer. He wants to be useful to humanity, but does not know how to achieve this. At any rate he wants to quit trade—suddenly he looks upon trade as some sort of robbery. He gets rude towards his customers, wants only to sell pictures he admires himself, and does not even answer when they ask for something different. He is getting cross and rude and neglects his clothes and appearance. From this time his development took another direction, this disillusion in love precipitated a *bend* in the evolution of his personality. At any rate, a person who reacts in this way hides under his seemingly healthy surface weak points, "complexes", which we shall presently see emerging more and more. We may see here the first slight "Schub", the first attack of his mental disease. The graphologists, Rose and Mannheim (2), however, who found typical schizophrenic deviations in his handwriting, could show the first symptoms of these already, shortly before his proposal. Therefore disillusion over the refusal cannot be the cause of the change in the handwriting. But this change might be connected also with the emotion and uncertainty of this first love, as well as with an internal process that might be secretly developing.

"My tendency to draw has stopped" he writes to Theo a short time after the catastrophe. He seeks some substitute-satisfaction, not only in religion but also in much reading and much smoking. His letters to Theo become gradually more frequent, longer and more detailed. A remarkable difference ensues between his letters and the other, more direct, expressions of his personality. In his letters he is tender, sensitive, full of a sense of duty, extremely religious at this time, but always civilized—in practical life he is intractable and hot-tempered, often neglecting the simplest everyday duties.

We should not conclude that his letters are in some way hypocritical; on the contrary, we must regard the phenomenon as a beginning of the splitting of his personality, which is to manifest itself much more clearly later.

In this time of failure and frustration he quotes from Ernest Renan this formula of his aim in life: "Réaliser de grandes choses—arriver à la noblesse et dépasser la vulgarité où se traîne l'existence de presque tous les individus." (To achieve great things—arrive at nobility and emerge above the vulgarity through which the existence of almost all people drags itself.) Certainly he has reached this in his life. But we cannot read from this those virtues of modesty, complete unselfishness, and lack of ambition, virtues which many of his admirers ascribe to him, picturing him as a kind of saint, only inspired by his love of other people and his art!

He lost his job, and in 1876 went to England as an assistant in a boarding school in two small towns, where he also taught French and German and served as a curate.

His mental attitude in this period is mainly characterized by a growing bigotry, and also a growing repression of sexuality. When Theo writes him he has fallen in love with a girl whom his parents do not like, Vincent sends his brother a devotional reproduction: "I enclose the picture of 'the Huguenot', put this in your room, you know the story—a young man is warned by his

lady-love on the eve of St. Bartholomew's night—she wants him to wear the sign by which the Catholics will recognize each other: a white ribbon round the arm. But he refuses to do this—his Creed and his duty were greater things to him than his girl."

As in many cases Vincent's attitude towards God was determined by his attitude towards his father. God is clearly a Father-symbol; the attitude which the child had in respect to his father is transferred to God. In this period the adoration of his father runs parallel to his piety. Thus he begins a letter: "Flügel Flügel übers Leben (Wings, wings over Life), Flügel über Grab und Tod (Wings over grave and Death), that is what we need. Don't you think that for instance, Papa has this?"

Also it is obvious that his image of God bears Father-elements. He says for instance: "You say I am sad and alone, yet I am not alone, because the Father is with me." In a sermon, which he preached in the school at Isleworth he compares life with a journey from the mother's heart to the father's arms of God.

Again and again he compares God with his father and somehow identifies them with one another like this: "Sincere congratulations on Papa's birthday. That is a beautiful text for this day: 'Faithful is he who calleth you, who also will do it'—What it may be for our father and for us, we do not know, but we can in some sense leave it to Him, whose name is Our Father." There are numerous similar instances in his letters. The next failure in his career is as junior clerk in a bookshop at Dordrecht. Here he goes to the shop in an old black top hat of his father's, an absurd piece of behaviour of which he would have been incapable in his healthy years. And also, this otherwise so refined and sensible man showed from time to time a rudeness and indifference to others in sharp contrast to the rest of his personality, for instance he ruined his landlady's new wallpaper, fastening small reproduction prints on it with thick nails. His simultaneous rudeness and gentleness of feeling speak of a break in his character of which we shall see many other symptoms.

Then he takes a new step to identify himself with his father. He wishes to become a minister of the Faith, to be completely "about his Father's business". First of all he wants to pass the state-examination required to get access to the university. He wishes to become a "Sower of the Word". He goes to Amsterdam, hires a room, and takes lessons in Latin. More and more he wallows in sentimental bigotry. On Sundays he goes to church six or seven times, he prefers composing sermons to making Latin exercises. He tries to identify himself with Christ, a Father-symbol as well as God, by immersing himself in the *Imitatio Christi*. He chastises himself with a club, sleeps on a plank, eats dry bread and feels himself unclean. The meaning of this uncleanness is clear, his sexuality cannot be destroyed; this he experiences unconsciously, and consciously feels it as uncleanness. Meanwhile ascetic behaviour of this kind is stimulated by s.c. masochistic drives, by a lust for suffering. Once he produces a typical symptom-action: he throws his gold watch and his gloves into the collection bag in church. Certainly we must see this as a symbolical offering of a piece of himself to God. Those parts of his body and psyche which he fights so bitterly, in the first place the genital part, are here thrown away in a symbolical auto-castration.

Another peculiarity he shows in this period is his continuous and passionate writing.

He copies numerous sermons and calls this studying, His sister tells us: "He wrote so much and so swiftly that the letters on those closely-written sheets

are undecipherable—inkstains without sense, nothing more.” This urge to write, often only nonsense-scribbling, is well-known to us from other cases. There is often no special urge to communicate something to others, nor to write down something as a memory-aid for oneself—it is above all the simple urge to use and move the hand. For these people their hand is “libidinized”, and hand-functioning can give some symbolical discharge to their pent-up eroticism. Their hands have a special sensitivity. This can express itself in all kinds of pleasure-giving manual work, not only writing, drawing, painting and modelling. Therefore it is not surprising that we often find painters who are at the same time distinguished authors and many painters discharge their conflicts also in diaries. Vasari, Delacroix, Leonardo, Alfred Kubin are well-known examples. Goethe was uncertain for a long time whether he could express himself better by writing or by painting. Many art-critics and art-historians are artists who have failed in their career and tried to express their artistic temperament in writing about art. Undoubtedly van Gogh has ventilated his hand-eroticism in his thousands of letters as well as in his tremendous urge to paint. So it is understandable that in this period of repressed sexuality and forced negation of genital instincts, his pent-up energy found an outlet in superfluous writing. In this connection, the fact that he chose his gloves as a symbolical offering to God may also be meaningful—he clearly tried to rid himself of his burning pent-up eroticism, in this period mainly fixated to his manual function.

Suddenly after another year, in 1878, he declares that he wants to preach the Gospel directly, as Christ himself did. He wants to become a missionary in the Borinage, a notorious mining district in Belgium. This was possible after a 3-months’ training course in Brussels. Once more he fails; after a 3-months’ training period he cannot get an appointment. His eccentricity, his queer manners and appearance, his obstinacy and absolute lack of submissiveness are insurmountable obstacles. What a different person he is now from the young Vincent in London, before the “bend” in his development!

He resolves to go to the Borinage without an official appointment at his own risk. There his situation gets worse and worse. He gives away all his possessions, is dressed in rags and filth, does not wash himself and even deliberately soils himself to look as filthy as possible. He tries to preach a bit—without success—and people look upon him as a lunatic. He confesses imaginary sins to miners and punishes himself publicly for these sins. He secured after all a provisional appointment, but lost this because he was declared insane and unfit. He does not care much about this: “Christ also stayed calm in the midst of the Storm”, he says, and he stays where he is. He lives in an empty hut and sleeps on a heap of straw. Theo writes to his parents: “Under his fine words he hides a vegetative lust to suffer.” He does not answer his parents’ letters. Sometimes he just wanders, vagabondizes, sleeps in the open or in a hay-stack. Everybody knows that afterwards in France he suffered from a grave mental illness, but it is not generally realized that in the period in the Borinage he was quite as gravely ill mentally, only the symptoms were different. In the meantime a new turning point in his life begins to shape itself. As the church-authorities are rejecting him he develops a negative attitude towards the church and his anger extends itself also against religion itself. He has a great hunger for love and he writes eloquently. “Tel un grand foyer dans son âme et personne ne vient jamais s’y chauffer (Here is one who has a big fire in his soul and nobody comes to warm himself by it).” He does not write about religion any more, but a great deal about pictures. Finally, on the 24 September, 1880, he writes the moving statement:

"Nevertheless it was in this deep misery that I felt my energy return, and I said to myself: Whatever may happen, I will surmount these obstacles once more. I will pick up my pencil again, which I dropped in my great discouragement, and will take up drawing again. From this moment on, everything has changed for me."

Thus in his 27th year he devotes himself definitely to art, "that big fire in his soul, his burning need for love, makes an artist of him", wrote Meyer-Graefe (3) truly. It was really his pent-up libido, his great desire to love and to be loved, which made him find this way.

Trade and intellectual pursuits, which he followed for a short time, were far too realistic to give him a satisfying outlet for his unconscious complexes. It is true that religion could give an outlet to his father-complex, but Protestant religion is so very much spiritualized—hardly sensual—and better suited to act as an agent to repress sexuality than to express it. But where religion could thus bring him no real solution, art could do that much better. Art possesses a natural sensuality, is nearer to nature and the unconscious can express itself much better in an individual way through art. Herein his libido could direct itself towards real form, and instead of the personal contact with people he could keep himself in the background and work for himself alone, the result being his pictures, which he could give instead of himself. Art gave him an ideal solution to the great controversial trends in his personality: his narcissism and his strong Ego-Ideal to be altruistic, his tendency to introversion and the sometimes equally strong tendency to be an extravert, his need of contact with other people and his aversion towards such contact, through art he could achieve all this alternatively or even at the same time.

After a short stay in Brussels, where he took drawing-lessons, he went home to live with his parents at Etten, another village in the province of Brabant, but presently some new conflicts were added to his old ones. Parallel to his negative attitude towards religion he developed a negative attitude towards his father. And here at Etten he experienced his second great disillusion in love. He fell in love with a cousin, a young widow with a child who was staying with his parents. At first this new love gave him some positive revival, like the first one. He wrote: "Once more I am happy to be alive, and I am very glad to be in love. My life and my love are one!" And when she refuses him with a "Never-no-never—" he says, "This answer is for me only a piece of ice, which I press against my heart to melt it with my warmth." This warmth of life, this ardour which he feels in himself is real enough, but his reaction to the whole episode is again pathological. He could not accept her refusal, kept asking and troubling her until she refused to see him any more. Then he travelled to Amsterdam, where she lived, and annoyed her father, his uncle, who would not allow him to meet her again. To show them the intensity of his love he held his hand in the flame of a candle, until the smell of burning flesh filled the room.

Again we see here his tendency to sacrifice a piece of himself, now not the glove as symbol of the hand, but the hand itself, as an essential part of his personality. His hand was refused—so he wants to throw away that hand. Consciously he wanted to show his love and constancy, unconsciously he was surely driven by his anger against his uncle, and also probably again by his tendency to seek pain, to deliver himself masochistically to the fire. When his father, after the scene, refused to give him money to travel to Amsterdam again, he made a violent scene at home. After this he left his parents' home and went to the Hague in December, 1881.

From now on Theo paid all his expenses and took over the positive father-part permanently. For Vincent he became the ideal Father, and Vincent calmly took it for granted that Theo should support him completely, though Theo really could not well afford to do this.

The period at the Hague is not very important in the development of Vincent's personality, though artistically he learned a lot in these years. His negative attitude towards his father was transferred to all authoritative persons he met there; thus he rebelled rudely against his teacher, Mauve, then a famous painter, whom he admired and loved at the same time.

He took up a relationship with a former model, a vulgar woman with a child, ugly and degraded, whom he at first idealized, but who infected him with gonorrhoea. Of course she could not give him what he needed. The situation became impossible and he left her and the Hague. After some equally unsuccessful months in the province of Drente he sees no alternative but to return home to his parents, who are now living at Nuenen. He had been living entirely at the expense of Theo, but Theo himself cannot continue to support him.

Here at Nuenen he leads a double life. On one side there is his art, to which he devotes himself with great intensity, and his admirable letters to Theo, exquisite expressions of a delicate, artistic and idealistic mind. On the other side we see an impossible person, always quarrelling with his parents and others, rude and disagreeable. In the beginning he has his studio at home, but he estranges himself more and more from everybody and lives a lonely life full of bitterness and self-imposed poverty, sometimes existing for weeks on dry bread and cheese alone.

Sometimes he wanders for hours through the fields, by preference in rainstorm and cold weather. And on his return home he sits in a corner of the room, his back turned towards his parents and casual visitors, without speaking, just reading or eating something. When anyone asks him something he does not answer. Many people who knew him in this period again considered him a lunatic.

Theo once wrote about him: "He seems to consist of two different persons: one wonderfully gifted, sensitive and tender, the other egotistical and heartless. They show themselves alternately, it is a pity that he is his own enemy."

When his father died at the age of 85 years, Vincent's grief was great and real. In spite of his negative attitude towards his father and his permanent quarrel with him, Vincent unconsciously was still much attached to him. After his father's death he soon left his house, this time for ever. Yet he stayed at Nuenen for another six months, a period in which he was very productive. Then he left for Paris, where Theo lived. After a short stay in Antwerp he joined this younger brother who was always his second father.

Before we follow him to Paris a few words must be said about his art in his Dutch period. What did he try to express in his works? His own answer to the question is eloquent: "I want to make drawings that will touch *some* people—I want to reach the point that people will say of my work: 'That man shows deep feeling and he is sensitive'—that is my ambition. I want to make drawings that are *not* understandable to everybody—the figures simplified, only essential traits expressed."

The writers who describe van Gogh as a kind of socialistic "all men-are-equal" idealist, basing this view on some other words of his, should also consider these words. Here speaks an aristocratic mind, whose first object is to express clearly in his work his own values. His main object is even to make drawings which *not everybody* will understand, but which will make an

impression on *some* people. But what about the subjects he drew and painted? Often this period is called his realistic period, but unjustly so. The poor peasants he represents are miserable, misshapen toilers, apelike in their ugliness and degradation, bestial slaves of life, with hollow eyes in starved faces and skinny hands like claws. There still exist some photographs of his models for the famous "Potato-Eaters"; they were not nearly so ugly and miserable-looking as van Gogh represented them. Quite as pessimistic and sombre as his subjects are also his colours in this period—black, brown and dark-grey predominate.

The short period in Antwerp was a transition episode to the Parisian period, both in artistic and in psychological respects an important step towards greater freedom. I want to point out two important factors in his development during this time: his acquiring of the female naked figure as a painting subject and his confrontation with Rubens. In Brabant he never had painted or drawn naked models, had indeed hardly found any opportunity to do so. At the art school in Antwerp he visited, only male models were permitted to pose undraped. But Vincent succeeded in becoming a member of a painters' club where female models were hired, and strangely enough, here he tried to paint with flesh-tones somewhat resembling those of Rubens's female subjects. Already in his last letters from Nuenen he wrote that he was longing to see Rubens's work—a striking change from the atmosphere in which he lived before! In his attempt to imitate Rubens's flesh-tones he showed for the first time a tendency towards a lighter and clearer palette. Also the fuller forms in which Rubens expressed his sexuality attracted him. When at an art-school lesson he had to make a drawing of the Venus de Milo he clearly thought her too slender, and in his drawing he over-accentuated her hips and transformed the Greek beauty into a robust Flemish matron. When his teacher tried to correct these forms with some pencil-strokes, Vincent exploded: "You don't seem to know what a woman is, damn you! A woman must have hips, buttocks, a pelvis where she can keep a child."

In Paris a fundamental change and improvement quickly developed, very striking to everyone, psychiatrist or layman, who has followed Vincent's evolution until now with interest and concern. Theo wrote home in the summer of 1886: "You would hardly recognize Vincent, he has changed so much, he is much gayer than before and everybody likes him here." Two factors may have caused this change, Theo himself and the new freedom. For the first time since his childhood he was now permanently near Theo, the human being whom he loved most of all in the world. In Theo he found again his once adored father but without his father's moralizing tendency. Theo gave him the warmth and protection of his parent's home, but totally freed from the fettered atmosphere which imprisoned him. His parents, though liberal enough in a way, could be very narrow-minded.

Vincent himself tells us how they abhorred everything "French", and "French" meant to them and to their friends "immoral", and he tells how they read Goethe's *Faust* only when the parson-poet Ten Cate had sanctioned this doubtful work by translating it into Dutch; but they looked upon it merely as "the disastrous result of an indelicate love".

But here in Paris he was delighted by a new freedom. He found a circle of friends in the younger impressionists who stimulated him immensely. He felt free to speak and to think as he liked, to drink alcohol and mix with women, to dress as carelessly as he wished. Here at last, he realized, what he had written before, feeling himself then a caged bird: "What opens the prison

is deep-earnest affection. To be friends and brothers, to love, that is the way to open the prison."

Already in earlier years the rare meetings with Theo had given relaxation to his pent-up libido. And when at the Hague a small shadow had occasionally threatened to fall over their friendship, Vincent had felt each time positively ill.

Thus he grew completely receptive to Theo's impressionist friends, turned from his dark way of painting and took over the clear gay colour of the Parisian impressionists. Now these colours were also in harmony with his mood, with his improved psychical state. We see daily in our profession how a new love, a new friendship, and a liberation from moral views can be an enormous healing force to neurotic patients. Yet in this case we see something truly exceptional, a person, not only on the verge of a grave mental disorder, but who has perhaps already definitely overstepped the boundary between sanity and insanity, who all at once is influenced so strongly and positively and regenerates so fundamentally. The agent of this exceptional recovery must be both his still potent possibility of extraversion, and the lucky circumstance which relieved one of his most damaging complexes—his father-fixation, which he could elaborate better through Theo as a medium.

Yet we must not expect that there in Paris Vincent was all at once changed into a harmonious person, that the well-adapted and well-balanced young man from London and the Hague had returned. A deep cleavage separated him from this early self, a different character had developed. But here he did not seem so very eccentric.

Alas, this considerable improvement did not last very long. Probably the intimate living together with Theo in a very small apartment, as well as the too intimate contact with other friends, irritated his repressed homo-erotic tendencies. He writes himself about this conflict: "If I had stayed in Paris, Theo and I would have become too much immersed in one another." Perhaps also his once activated introversion was a process which could be stopped for a short period, but which inevitably and spontaneously evolved in the long run. During the winter of 1886 he became more troublesome to others than ever, and made life difficult for Theo, whose own health was precarious. In the spring of 1887 a fresh improvement occurred, probably caused by a new friendship, with a very young painter, the 19-years-old Emile Bernard, who admired him very much and never contradicted him. But once he had a quarrel with Bernard's father about Emile's education and from that moment he never entered the Bernard house again, and during the following winter he was much worse again. He did not feel so well physically either, the noises of the city troubled him and he had a frightening sense of a dark menace, an undefined something in himself that was growing and rising, so that later in St. Rémy he wrote to his sister: "On the whole I prefer to have a definite illness than to be as I was in Paris, when this was smouldering." Also, he suffered from anxiety-dreams.

Paris now appeared to him cold and dark, a hot-bed of ideas, "but nothing is really fresh here", he wrote to his sister; he was longing for nature, for light and warmth, for the sun. To find all this and perhaps also to run away from contact with other people, who excited his complexes too much, he went to the South of France in February, 1887, to Arles in Provence. Once more at first he attained a remarkable recovery. As a painter he rose here to his greatest height. The two contrasting sides of his personality, the lover of nature of Holland, and the lover of light of Paris had as yet not reached a synthesis in

his mind. But the burning sun of Provence gave him the warmth he needed, and fused and melted together the still conflicting elements in his art.

At the same time a very important psychological process developed. Vincent experienced an intensive mystical union with nature, especially with the sun. He evolved a regular sun-cult and sun-worship.

He got into a kind of ecstasy, a state of mind which appears to me to have been some kind of healing-effort, a tendency emerging from his unconscious nature. Undoubtedly this was a regression to those well-known primitive experiences of oneness and undividedness, which in extreme cases can result in ecstatic conditions. For Vincent this meant a "*reculer pour mieux sauter*". He writes about this: "Often I do not know what I am doing, I am working almost like a somnambulist", and also he speaks of "a lover's blindness for my work" which possesses him. Two striking features characterize these ecstatically painted pictures, the ever-present sun and the all dominating yellow colour. He seeks the sun, and in the sun he reaches his greatest ecstasy, and creates his best works. What did the sun mean to his unconscious? I know no better answer to this question than that of the German art-critic Hartlaub (4): "The sun was for Vincent the cosmic mask of God, animating all nature, uniting him with nature and with God." Apparently big words, yet the simple truth.

Similar experiences of being united with God and the world are well known from many mystics in conditions of changed consciousness as well as from many psychotics: In these conditions we see a diffusion of Ego boundaries; these mystics feel themselves part of the world, the sun, the sea or God. In Vincent's case primitive religious experiences emerged, the sun was for him a symbol of God, of the Father, and at the same time of Theo. As Meyer-Graefe writes: "Theo was at the same time his brother, a real and a mystic person. Instead of Vincent and Theo, we can put Vincent and the world, or even Vincent and God." We may add to this: "Vincent and the Sun."

The yellow colour from now on so prominent in his pictures is for him the colour of the sun—the sacred colour—the colour of burning love which he can now at last pour out profusely into his art. Emile Bernard relates how Vincent painted the *Berceuse* as a symbol of Mother-love for lonely sailors; on both sides of this picture should be put two paintings of big yellow sun-flowers, their yellow colour symbolizing Love's omnipotence.

He used freely contrasting complementary colours, a technique often practised by the neo-impressionists with their unbroken colours. But he put a lot more meaning into this practice than was contained in impressionistic theories. He wanted to "express the love of two lovers by a marriage of two complementaries, their mixture and opposition, to express hope by some star, the ardour of some being by the rays of the setting sun".

The complementary colours yellow and blue-violet, red and green, he used mainly to symbolize the male and female principles; they were to him "the marriage of the complementaries". The woods and mountains he painted were subjected passively to the active radiation of the sun-eroticism. In the more primitive attitude towards the world Vincent himself was sun and earth—man and woman. At one moment he is male like the sower, and sows the yellow colours over the picture, at another he is female like the earth, which receives the seed, as his body passionately receives the sunbeams.

So Art could bring him, be it temporarily, a recovery, and his logical Ego, with its theories found therein as much satisfaction as his ideals and his love for nature; at the same time his repressed erotic tendencies could find some outlet here. On this (partly somnambulist and primitive) stage, he found a

psychic harmony, all his pent-up forces could express themselves here together. In this period he created his best works.

All too soon, unfortunately, the harmony was shattered by his well-known dramatic collapse at the end of the Arles period. In many descriptions of Vincent's life the authors try to attribute this collapse to causes like overworking, too much sitting in the sun, heavy smoking, under-nourishment, etc. To the psychiatrist such explanations are hardly satisfying. But a short time before the crisis two very important events happened, namely: (1) Theo's engagement to be married, and (2) the difficulties between Vincent and Gauguin, who was then with him at Arles. The bond between Vincent and Theo was so strong and so deeply embedded in Vincent's unconscious, that clearly there existed something like a psychic marriage between them, and his pictures were for him the "children" of this union. In fact he remarks repeatedly that they had again produced "together" a new picture. All his creating was for Vincent a being together with Theo. More especially he speaks about "accoucher d'un tableau" (being delivered of a painting). He signed his pictures only by his Christian name: the explanation he himself gave was that in France the name Van Gogh was unpronounceable, but this is certainly not correct as already in Holland he signed only as Vincent. Surely we have to take into account here that for himself and for Theo he always remained the child Vincent, and that in Love and in Art he always lived for Theo, so that it would have been an absurdity had he entitled himself before him (Theo) as V. van Gogh.

Although Vincent consciously was glad that Theo had found the love of a woman, the happiness of a marriage, it is inevitable that from the moment Theo got a beloved wife to love and cherish, Vincent must have felt himself abandoned. All the more important must have grown for him another project, long cherished by him. He hoped to found with Gauguin and other friends a society of painters, a sort of league of comrades. The most important feature in this was for him the libidinous tie with what he called "les copains" (the pals). Significantly he wrote: "Human beings are more important than things, and as for me, the more trouble I take with my paintings, the more the paintings themselves leave me cold, the reason why I try to make them, is to be among the artists." All his hope was fixed now on Gauguin. He idealized and worshipped Gauguin in a foolish way, called him "Master" and hoped that he would consent to act as head or father of the herd of neo-impressionists—briefly he had a father-transference on Gauguin. But Gauguin thought Vincent a romantic fantasist. When Vincent got the news that Theo was engaged to be married he once more gave Gauguin a detailed account of his plans and ideals and asked him to co-operate. But Gauguin thought these plans ridiculous and roared with laughter at them. This ruined Vincent's last chance of retaining (psychical) stability. Having lost every possible outlet for his pent-up libido, inevitably hostility against Gauguin had to come out. The next day in a cafe Vincent threw a glass of absinthe in the direction of Gauguin's head, without any clear reason, Gauguin brought him home, where he fell asleep. The following day Vincent, vaguely remembering that he had somehow insulted Gauguin, asked his forgiveness, but in the evening he ran after Gauguin in the street with a razor in his hand. Gauguin turned round and looked him sternly in the eyes, whereupon Vincent bowed his head and slunk away. The same evening he cut off an ear-lobe, wrapped it up neatly in a piece of paper and brought it as a present to a prostitute in a house he often visited. The next day he was taken to the hospital.

Here we see, doubtless, the well-known mechanism of an aggressivity

checked at the last moment and then directed against his own person. Had he made a simple attempt at suicide, we could have easily understood it, but to cut off an ear-lobe and then bring it to a prostitute is an insane action. We must look upon this as a symptom-action, psychologically resembling closely what he did in Amsterdam, when he sacrificed his watch and his gloves and burned his hand in a candle-flame. Undoubtedly, what brought him into this situation, besides his excited aggressivity and his self-punishment tendency, was his pent-up eroticism. His love for a woman was frustrated every time, he had been compelled to suppress it and had tried to sublimate it in his art and in his contact with his friends. It is true that he had frequent contacts with prostitutes, but that had only been a superficial attempt to find relaxation, without any deeper meaning for his inner life. Also, we saw how in ecstatic conditions all his libido was directed towards God, his father, Theo and his friends. When he was losing Theo and Gauguin, his inner balance was so gravely disturbed that an attempt to sacrifice his libido seemed a logical reaction. But once more this sacrifice took the form of a dream-like symbolical action, clearly a symbolic attempt at auto-sterilization; the ear-lobe was a symbol for the male genital, which he brought to a place where it would be accepted.

After a short stay in a hospital he returned to his work, but he was no longer the man he was before. From time to time he had twilight-states with anxiety, hallucinations and religious delusions. In one of his last attacks which necessitated his admission to the asylum at St. Rémy, he threw several paintings out of the window to the crowd, crying out with a schizophrenic-like pun which he had also written on the wall of his room, "*Je suis le Saint-Esprit, je suis sain d'esprit*" (I am the Holy Ghost, I am sane in mind!). One night he sat painting in the open with a crown of burning candles on his hat. And the children from the streets hooted: "*fou roux, fou roux*" (red-haired madman). Before describing the end of the evolving tragedy, we have to examine his religious experiences. We have seen how he had turned away from religion (at least consciously) and neither his way of living nor his ideas could be called "Christian" in any way. But unconsciously the religious attitude stayed active within him, emerging again in his delusions.

Even in his non-psychotic phases his attitude towards Christ was remarkable, for instance he writes about Him thus: "He lived serenely, a greater artist than all other artists, contemptuous of marble and clay and colours, working in living flesh; he created living men, immortals." And he finishes: "His spoken words are the summits reached by art, which in these words becomes pure creative power."

For him Christ was the artist working in living flesh and by the fertilizing force of his words He became the personification of the highest source of art, the Libido. Undoubtedly in his Unconscious the infantile relationship towards an overpowering Deity, on the one side with father-qualities, on the other magically merging in experiences of libidinous energy, remained of great importance. And this unconscious religion directed his delusions, of which he once said: "I can't understand how a man with such modern ideas as I have can have states in which I have confused and cruel religious ideas, such as I never had in my worst days in Holland."

A strange fire blazes through his paintings in these days and there is in the different figures a peculiar unity with their surroundings, all consisting of revolving, whirling lines; and certainly in these works he always painted himself, his pent-up creative power. This new expressionistic style was a direct

effect of his greater introversion and at the same time of a more direct activity of his primitive mode of reality experience.

Some critics pointed out that Vincent always avoided painting Christ as a person. In the asylum of St. Rémy he produced paintings after etchings by Rembrandt and Delacroix, transposing them into colours; in a curious way he translated the chiaroscuro of these etchings into different hues of colours. Remarkable is his version of the Rembrandt etching, the Resurrection of Lazarus, in this picture the dominating majestic figure of Christ is left out and replaced by the Sun shining over Lazarus.

Only once did he paint Christ. He transposed in St. Rémy a pietà of Delacroix in colours. But here Christ was not the Saviour, but the Man of Sorrows, a completely helpless figure, held up by his mother. And the face of this Christ clearly has the features of Vincent himself—it is a self-portrait. The mother-image had not come to the fore in his conflicts as the father-image had done, it meant to him only the rest before and after all the struggle. In the pietà, that is only in death, he would find that rest. And in the last, difficult days at Arles, the mother-image emerged too and he struggled to find an expression for it in the picture of the Berceuse, of which he made five versions, all much alike. Emile Bernard writes how Vincent painted the Berceuse as a symbol of Mother-love, and his wish had been that sailors, “at the same time children and martyrs” (like himself!), might see it in their cabins and then “experience a rocking feeling, reminding them of the song of their nurse”. The dubious beauty of her yellow face is not to be explained by jaundice of the model, but by the mental disease of Vincent. In a non-realistic, symbolic way of feeling, yellow was to him, as we have seen, the colour of love. And it is very questionable whether any sailors might ever experience any “rocking feeling, reminding them of the song of their nurse”, looking at the picture of this unattractive, introverted yellow woman. Before being able to understand it, we have first to learn that the love lies in the yellow colour, and that the motherliness and the rocking have to do with the cord in her hands (because the cord can be thought to be fastened to a cradle). The natural direct contact between artist and spectator is loosened here, and our thoughts have to make a construction to bridge the gap before we can empathize with Vincent.

In the asylum of St. Rémy the attacks returned, and in the intervals he was more unstable than he had been before. He seriously considered joining the Foreign Legion, and had horrible nightmares. Sometimes he tried to swallow his colours and the materials producing them. Some authors have interpreted this as a suicidal attempt, but I am convinced they are mistaken. In this respect we must remember the libidinous meaning his colours had for him, and his tendency to introject and to unite with all he loved. In his deep regression he realized this also in this most primitive, oral-erotic way.

Charles Mauron (5) in an interesting and accurate study was able to establish a relation in time between his attacks and some unwelcome news in the letters he received from Theo, and Kraus has shown how bad news about Theo's health (far from good in this period) undermined him.

Next to the image of the Sower that had possessed him so passionately and had been for him a symbol of the fertilizing germinative power, of libido, of life, now that of the Mower came to the fore. “I saw in this reaper”, he wrote “the image of death, humanity being the corn he reaps, he is the opposite of the Sower. But in this death, no sadness, I sought the ‘presqu'en souriant’ (almost smiling).” This last expression refers to words said about Delacroix, which Vincent often quoted: “Ainsi mourut presqu'en souriant Eugène

Delacroix, peintre de grande race qui avait un soleil dans la tête et un orage dans le coeur" (Thus died almost smiling Eugène Delacroix, painter of high pedigree, who had a sun in his head and a storm in his heart). And the tree of death, the cypress, became dominating in his pictures and he himself saw it as the counterpart of the sunflowers.

At the beginning of 1890 he went to Auvers, under the care of Dr. Gachet. Curiously enough Vincent also sent Gauguin's bed, from which he could not separate, to Auvers. There he became still more depressed. Kraus has shown that this must have been reactive to letters from Theo, telling him something of Theo's failing health. The pillar he relied on throughout his life staggered. "Je reste toujours toqué (I'll always remain crazy)" he wrote, and he felt that the attacks would return. His last painting, black crows in a wheatfield, with its threatening, oppressive sky, with the crows, the birds of death, flying along, tells of the approaching end. His internal disintegration is reflected in this painting in two crushed setting suns.

On a Sunday morning in July he fired a bullet through his body. So now he sacrificed *himself* to the glorious world and the shining art, preferring this to sinking down into permanent insanity. Instead of the Sun, that demoniac symbol of God, Father and archaic Love, he chose to give himself up to the mother-earth, the Pietà. The very last day of his life, he passed quietly smoking and talking to Gachet and Theo, almost smiling, and dying (on the third day) in the Café de l'Espérance. And the words which he so often quoted about Delacroix are pre-eminently appropriate to himself: "Ainsi mourut presque en souriant, V. van Gogh, peintre de grande race, qui avait un soleil dans la tête et un orage dans le coeur."

We have seen in what preceded a parallelism between his psychological and his artistic evolution. We have understood that a consequent realism and impressionism (the latter endeavours, too, to paint nature in an objective way) could not appease his craving for self-expression, so that in due time he inevitably had to come to an expressionistic way of painting. All his pent-up libido, his stirred up unconscious, pushed to the surface and created strange distorted forms. He was the first expressionist, and he became that because he had no choice. His way of painting was not the result of an intellectual theory, but an instinctive creation. In a witty way Jaspers (7) has said: "In the exhibition at Cologne in 1912, where the wonderful van Gogh's were surrounded by expressionistic art from Europe displaying a remarkable monotony, I often had the feeling that van Gogh was the only grand and involuntary crazy person among so many who apparently wished to be crazy and are only too healthy." Vincent's work is more deeply rooted in the vital sphere, it is biologically founded and woven together with his whole life-history, his conflicts and his psychosis. But this expressionistic solution of his conflicts was in fact a going to the utmost limit where a real art still was possible. A further shift of the inner energetic relation could become fatal for the artistic result. He stood on the brink, and he wrote himself in a quiet phase: "Some of my paintings bear the features of the sick person that made them."

Did he cross this threshold in the end, and do his works show any signs of decay? Some critics think they do. Thus F. Knapp (8) writes: "His greatness is finished . . . in the weak, loosened way of handling, in the dissolution of reality and in the somewhat sugary colours we may discern the disintegration of his mental forces." The unity of composition generally is preserved, at least in St. Rémy; from the burning, beaming, swirling air, houses and trees there radiate rays and circles that unite sky and earth into one image of the all-

animating and all-penetrating force of nature. But still there are among his later works several that seem to us more queer and distorted than aesthetic. Moreover, sometimes it looks as if the circles, spirals and revolving lines are beginning to lead a life of their own, whereby they seem to dominate the whole composition. Ornamental traits and patterns appear, and we wonder whether we have here before us, perhaps, unbridled tendencies, forces, repeating themselves stereotypically and somehow emancipating themselves. And with concern we ask ourselves what kind of development we might expect here—especially when we know how with some schizophrenic artists these stereotyped and ornamental trends can overgrow and stifle mental plasticity as an expression of the fact that only a few stronger tendencies got the whole of the psyche in a stiffening grip.

Finally, we have still to consider some questions about the diagnosis, although I consider the matter of labelling of minor importance compared with that of his psychological development and the history of his conflicting trends, in which we have accompanied him in the preceding pages. Diagnosis may be irrelevant now; important is the fact that all Vincent's disturbances have proved to be psychologically motivated, understandable reactions to situations that were traumatic to his sensitive personality, that they never proved to be automatic or autochthonous, and that they could be healed partially—although unfortunately not entirely—by other psychological situations, which he either sought for himself, or which were created by Theo's love.

All authors agree that his psychosis was not a typical one. So several psychiatric concepts are applied to his case and there is talk of "the seven diagnoses of Vincent van Gogh", as if these were as many keys that could unlock for us the entrance to his psyche. They cannot all be discussed here, but as some serious French authors still defend the diagnosis of Epilepsy, I must say something about this, although I wholly agree with Kraus, who wrote: "I consider the diagnosis of Epilepsy an absurdity." Vincent never had epileptic fits, but it is well known that on the periphery of that disease there exist periodic states without fits, wherein consciousness is severely disturbed and hallucinations may appear, whereas in the intervals the patient is quite normal with a complete amnesia for the sick periods. Kleist called them episodic twilight states and they are (furthermore) characterized by fits of severe frenzy, attacks of rudeness to other people and self-injuries. Riese and Minkowska believe that these characteristics exactly fit Van Gogh's case. I do not think they do. He never committed attacks of aggression on others. Probably Riese (9) was thinking of his running after Gauguin with a razor in his hand. But in the first place that act had a very obvious reason (whereas the episodic twilight states should appear without any motive, "autochthonous") and moreover it is nonsense to call this an aggressive "attack", as he did not even touch Gauguin and shrunk back simply because Gauguin looked sternly (squarely) at him. (I would not advise you to try that with an epileptic in his twilight state.) And Vincent's self-injuries never were "autochthonous" but always psychogenic reactions. And weakest of all stands the heredity argument that Riese and others advanced in favour of their diagnosis. In St. Rémy epilepsy was diagnosed because Vincent said that a sister of his mother and other members of the family had suffered from that disease. Probably nobody would ever have thought of calling Vincent "epileptic" had he not himself said so in a confused mood. However, this contention was absolutely false! Not a single epileptic fit ever occurred in his family, as Mr. van Gogh has assured me. The ladies in question were perhaps rather singular personalities, but nothing more. On the other hand it is

absolutely certain that Vincent's youngest sister was a genuine schizophrenic and vegetated for more than 38 years in an asylum. Of great importance, also, is the fact that Vincent did *not* have a total amnesia for his attacks; in a letter to his sister, published in 1955, he described accurately his sensations in them.

Another important question is whether he was "normal" in the intervals. Certainly that was not the case. "I feel myself wholly different from before", he writes, and when he was back in Arles for a short visit, all the people there seemed to him "beings from another world". Now and then there is a mild, quiet confusion. In 1890 Theo writes: "I got a letter from Vincent which again is totally incomprehensible." From St. Rémy Vincent writes how he gets on with other patients—"we understand each other very well, for instance I can chat with a man who answers only with incoherent phrases". But this man was an idiot, who had never learned to speak a single word. To speak to an idiot, who only utters confused sounds and then believe oneself to be very well understood—that sounds very autistic indeed.

Madame Minkowska has tried to differentiate what she calls the "glischroid" character of the epileptic from the schizoid character, and she assigned the "glischroid" character-structure to Vincent. For a few of his character traits this may seem right, for the rest not at all. The coincidence is, however, not very surprising, as Minkowska has moulded her description of that glischroid character-structure a good deal on Vincent, whom she considered a typical epileptic. ("Ich schulde dem Studium Van Gogh's das Verständnis der epileptischen Struktur" (10), p. 200.) But it is unquestionable that some of the most typical epileptic, "glischroid" characteristics are missing in Vincent. "Viscous, with a want of mobility" is the last thing that could be said of this sensitive man, any more than that he was a man "with little initiative, creating nothing new" and "without an individual touch" ("die Affektivität ist dickflüssig und zeigt einen Mangel an Beweglichkeit . . . ihren menschlichen Beziehungen fehlt die individuelle Note . . . sie haben wenig Initiative und schaffen nichts Neues", Minkowska, l.c. 186).

Latterly another variety of the group of epileptic diseases has come to the fore, temporal or psychomotor epilepsy. Although there exists, of course, no EEG of Vincent, and this diagnosis cannot be proved, Gastaut has argued that several of his symptoms at Arles and St. Rémy might fit into this pattern perfectly (14). We need not discuss them all here, for the main fact remains, that this theory cannot give a satisfactory explanation of Vincent's undeniably schizoid behaviour in London and in Holland. In taking the beginning of the disease at Arles, and so studying only the final period, one sees only a last offshoot of the disease and neglects the main and best-known facts. Jaspers, who labels him as schizophrenic, has the merit of seeing that already in Paris his mind was disturbed, but his mistake was to ignore the fact that the most important part of the disease had already taken place in Holland. This disease was considered by his family as serious enough to occasion repeated consideration of putting him under guardianship or placing him at Gheel (a village in Belgium renowned for the nursing of lunatics in family-homes). So we must either accept that he had two diseases, a schizophreniform psychosis in London and Holland and a psychomotor, temporal epilepsy at Arles, or we must see all the symptoms as belonging to one process, and then the choice is not difficult as all his earlier and later symptoms fit into the diagnosis of epilepsy, which is not the case with temporal epilepsy. Furthermore, schizophrenia is for us no longer what dementia praecox was fifty years ago, a wholly

incomprehensible psychosis with absurd, incomprehensible behaviour, caused by an unknown brain-disease. We have learned that all its symptoms are purposive, meaningful, and—in principle—understandable. And, as I have mentioned already, each of Vincent's abnormal actions, the outbreak of each attack, stands in psychological relationship to one or other psychological situation; never are we entitled to dispose of his symptoms by calling them "automatisms", as might be the case in a psychomotor epilepsy.

Of late, however, there are data that seem to bring some cases of schizophrenia into relation with temporal disorders. Erwin, Epstein and King (11), and others have observed cases where a typical schizophrenia was combined with temporal spikes in the EEG. Heath and co-workers reported "septal" spiking in the subcorticograms of schizophrenics. So it is not impossible that, if at Arles an EEG could have been made, some disorder might have been seen therein. But even so it certainly would not have been permissible to proclaim that the "cause" of the whole mental disease, being a damage of a special part of the brain had been found. For in the course of the years many physiological and even anatomical deviations have been found in different organs of schizophrenic patients, and were proclaimed as being the cause of this uncanny disease, but none of them has proved to be regularly present, not even in psychologically similar cases. Up till now schizophrenia remains a disturbance of the total psycho-biological personality in its communication with the environment, wherein, besides an hereditary disposition, damaging environmental influences, for which the patient is specially sensitive, play a role, and to which he reacts with regression and other defence-mechanisms. All this was there in Vincent's case. Schizophrenia being a serious and profound disturbance of the total personality (much deeper than most neuroses), not only are the reactions of the totality (i.e. the psychological reactions) disturbed, but also the reactions of differentiated organs (i.e. the physiological reactions), and more obviously than in neurotic cases. There certainly is what Rosenzweig (12) calls a disturbance of homeostasis, and in severe cases this may lead to anatomical alterations. Of course, such changes in brain-functioning might have had some secondary influence on the form of Vincent's psychotic reactions and may have co-determined the "psychomotor" features of his last period. But all this remains hypothetical, and we prefer to limit ourselves to the psychological facts.

One of the main reasons why psychiatrists again and again hesitate to label him as "schizophrenic" is his striking tendency to turn himself to the outer world, his warm and effective love for others, a love that is no less real, in spite of his strong aggressive, narcissistic and introversive tendencies. But we should not forget that extraversion and introversion do not exclude one another, that a man can be at once strongly introverted and strongly extraverted, that he can live in two forms of life at the same time, especially if his conscious ego is directed in this respect in another way than his unconscious.

Besides schizophrenia, there is yet another diagnosis that must be seriously considered and that is psychogenic attacks in a schizoid psychopath. Kraus formerly thought this quite possible. I could agree with the diagnosis of schizoid psychopathy, only if we consider that in London a mild schizophrenic process had befallen his non-psychopathic personality, and this process after remission left a psychopathized personality. In practice this question of diagnosis would have been important, for the prognosis is much more favourable in cases of psychogenic attacks on a psychopathic basis than it is in schizophrenia, as in later years psychopaths often do very well. But in diagnosing psychopathy not

enough attention is given to the fact that there is a gap between Vincent as a youth, when there were no psychopathic traits at all, and Vincent after the London period, after the bend in his development. And I am sure that any modern psychiatrist who could have had Vincent under observation, could only give an unfavourable prognosis.

In the last few years several cases have been published of severe schizophrenic patients who were treated with great success by a psychotherapy mainly based on bestowing on them an unshakable sympathy and devotion, of giving utterance to this in the right way, and of giving protection and co-operation on the level of the patient without any condemnation. The names of Rosen, Sechehayé, Getr. Schwing and others are connected with this therapy. Now with Vincent the almost unparalleled case presents itself, that he, properly speaking, always had a similar treatment from Theo, a treatment, however, that surely influenced very much the manifest form of his psychosis. Kraus (13) wrote about this: "What would have become of Vincent without the symbiosis with Theo? By his faithful friendship and protection, by his devoted help and support, in brief within the fence with which Theo surrounded him, a complex psychiatric figure was cultivated, that owes its rareness partially to the rareness of such a shelter." Really, I think that we have to thank Theo most of all for the fact that Vincent was not already submerged in his psychosis in Holland, like his sister, that Theo's care and love enabled him to unfold his capacity for extraversion and his biological tendency to recover again and again.

Vincent lived a tragic life—but I fear that a terrible tragedy indeed would have unfolded itself had he not committed suicide. For it was obvious that a permanent synthesis of his split-up personality was no longer attainable, and we shudder at the very thought of what might have happened in the future.

"Einigen sterben zu früh, viele sterben zu spät, noch klingt fremd die Lehre: Stirb zu rechter Zeit" (A few people die too soon, many die too late, strange still sounds the lesson: Die at the right time). The very man who said this, Nietzsche, died too late, but we are grateful to Vincent, not only for what he gave us in his work and his fervent life, but also for having drawn the balance at the right time.

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PERSONALITY CHANGES FOLLOWING TEMPORAL LOBECTOMY FOR EPILEPSY

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PREVIOUS papers from this hospital have reported clinical, neuropathological and other studies on patients subjected to temporal lobectomy for epilepsy (Falconer, Pond, Meyer and Woolf, 1953; Hill, Falconer and Pampiglione, 1953; Meyer, Falconer and Beck, 1954; Mitchell, Falconer and Hill, 1954; Falconer, Hill, Meyer, Mitchell and Pond, 1955; Meyer and Beck, 1955; Meyer and Yates, 1955; Meyer, 1956). The detailed psychiatric changes following temporal lobectomy are the subject of this paper. Apart from individual case studies and short comments, the effects on personality of temporal lobe ablation have not so far been reported.

The Montreal school who have had most experience with this operation have not studied the mental changes as exhaustively as other aspects, and they also have specifically stated that patients who show gross mental changes have been excluded from operation. Penfield and Flanigin (1950) reported that there was no alteration of behaviour, no particular memory loss and no persistent aphasia following operation, nor was there any defect of initiative or capacity for decision. Paillas *et al.* (1953) report briefly on the personality of four hospitalized cases; only one seems to have benefited from the operation; two others had a bilateral lobectomy without great effect to their disorder, but also without reduction of their intelligence. In agreement with Green *et al.* (1951) they consider overt attacks of rage a contra-indication to surgery. Improvement seemed to occur particularly in children, sometimes with an actual increase in intellectual ability. This is exemplified in the two cases reported by Liddell and Northfield (1954). Green *et al.* (1951) reported that patients with constant hostility benefit from the operation while patients with psychotic or psychopathic trends (including addiction) do not do so. Bailey and Gibbs (1951) considered that in the majority of their 25 cases, operation produced little change in behaviour. They noted that the inverse relationship between grand mal attacks and disturbed behaviour was more prominent after operation, although diminution of the psychomotor attacks following surgery produced less personality disorder than control by medical treatment. In the cases reported by all these authors listed above the excision of the temporal lobe has usually been limited to the lateral and inferior portions of the lobe anterior to Labbé's vein (i.e. the anterior half of the lobe). It has seldom included the deeper portions of the lobe, viz. the uncus, hippocampal gyrus, hippocampus, and amygdala.

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This may explain the differing results on personality reported by these authors and by ourselves.

PLAN OF INVESTIGATION

The majority of the patients in this series were referred initially to the out-patient clinics for epileptics at the Maudsley Hospital. Eight patients, however, came directly from Guy's Hospital to the Neurosurgical Unit. Twenty cases spent several weeks pre-operatively, and a similar or even longer post-operative period under our care in the Maudsley Hospital for intensive psychiatric observation and study. Histories were obtained not only from the patients, but also from relatives so as to obtain as complete a picture as possible not only of the medical events but also of the personality development and the changes therein that seemed related to the present illness. In addition to personality studies special attention was paid to the psychiatric aspects of the actual epileptic attacks—the content of the auras in so far as they appear related to significant features in the past life of the patient; the emotional precipitants of fits and the post-ictal mental changes. All this material was considered at special case conferences together with the results of the special laboratory investigations (EEGs, AEG, etc.), and the decision to operate reached as described in previous reports (Falconer *et al.*, 1955). In most cases the operation included the anterior 6–8 cm. of the temporal lobe including the uncus, hippocampal gyrus, hippocampus, and amygdala, but generally sparing the greater part of the superior temporal gyrus. Following the operation most patients remained in the neurosurgical ward for from 2–3 weeks and were then transferred back to the psychiatric ward for further study and rehabilitation. Subsequently, several patients whose mental condition showed fluctuation of disorder were re-admitted during the earlier part of the post-operative period of from 2–5 years during which this study has been made. The usual reason for such re-admissions was the development of a depressive illness.

CASE MATERIAL

Twenty-seven cases are considered here, since of the original 31 temporal lobectomy cases reported by our group (Falconer *et al.*, 1955), one suffered an early post-operative death, one was not accessible for follow-up study, one had a bilateral temporal lobectomy performed without improvement and with subsequent death from status epilepticus, and one was too young at the time of operation to have an established personality. Of the remaining 27 patients, all but one showed significant psychological disorder before and/or after temporal lobectomy. In these 27 cases there is an even distribution between the sexes and their ages range from 13 to 56 years. The longest "tumour" history (Case 19) is 22 years, but it will be seen that the duration of the fits is extremely variable. The pre-operative psychiatric status of the group will first be considered.

Pre-operative Psychiatric Status

Six patients had a history of epilepsy and 8 patients of mental disorder in their families. The latter includes only those families in which an individual had had an illness requiring treatment in a mental hospital, treatment at a psychiatric clinic as an out-patient or one who by reason of mental illness has been almost totally ineffectual in life. These figures are high and certainly

outside the range reported by authors for epilepsy as a whole. The early environment of 16 of the 25 cases in which it was known, was found on enquiry to have been seriously disordered by parental disharmony, quarrelling or desertion. In these cases alcoholism, discord, deprivation of the child of love and care, and separation from parents were most commonly noted. This incidence—16 of 25 cases, is again high for any group of psychiatric patients. Examples of stressful early environment are:

Case 15. The parents of this 25-year-old patient have never been in harmony at any time. Her father has always been a severe alcoholic, who just manages to make ends meet, and when under the influence of alcohol becomes abusive and physically violent. Constant quarrels took place throughout the patient's first 11 years of life, and at the end of that time her mother left to live with another man.

Case 21. A girl aged 22 lost her father before her second year. Her mother was left to fend for her five children in the worst phase of the economic depression, and barely subsisted on the dole and occasional char-ing.

The pre-morbid personality of the patients, that is, the personality before the development of epilepsy could only be assessed in 15 cases since in the remainder the seizures began before the patients were 4 years old. Of these 15, 12 had normal personalities before the onset of fits, as judged from interviews with parents or other relatives. The other 3 patients had well-defined psychiatric disorders—a paranoid state in one (Case 11), fetishism in the second (Case 12) and gross alcoholism in the third patient (Case 29).

Pre-operative Mental State

In only one patient was the pre-operative mental state regarded as normal (Case 16). The abnormalities seen in 26 cases included a range of symptomatology. The great majority showed chronic disorders of personality rather than persistent psychotic reactions. There were 18 cases of *character disorder* (Cases 4, 5, 7, 8, 9, 10, 12, 13, 14, 18, 20, 22, 23, 24, 26, 27, 30, 31), in which are included adults with psychopathic personality, children with primary behaviour disorders and other patients with fundamental disturbances of personality such as schizoid personality, ill-directed and excessive aggressiveness or rudeness or sexual perversion. There were three cases showing symptoms of *hysteria*, one showing serious phobias, the other physical conversion symptoms (Cases 22, 31 and 27). Predominantly *affective* disorder was seen in 2 cases, one with depressive suicidal wishes and self reproach which had previously responded to E.C.T., the other a mild endogenous depression (Cases 15 and 28). A *hallucinatory psychosis* had occurred in two cases, one with schizophrenic features, the other as a brief illness having paranoid character following a group of seizures (Cases 12 and 21). This patient (Case 12) also had a character disorder and has been reported (Mitchell *et al.*, 1954). Predominantly *paranoid states* were found in 5 patients (Cases 6, 11, 19, 25 and 29). This diagnosis was made in a person whose predominant psychiatric disability was the feeling that a part or whole of his environment was against him, who was able to give obviously delusional examples to justify his feeling and who showed resentment at the same time.

Effect of seizures in the pre-operative mental state. It has been held that seizures and the inter-ictal personality disorders have an inverse relationship. Excluding prodromal mental changes, this relationship was observed in 8 of 26 patients in whom this information was available. In 3 of these 8 patients the seizures *sometimes* did not affect the mental state and *sometimes* greatly aggravated the disorder. In the other 13 patients the disorder of personality and the

occurrence of seizures appeared to be independent of one another. The study of this group of patients therefore does not support the idea of an inverse relationship between seizure incidence and personality disorder. However, in other patients studied by the authors such an inverse relationship has been occasionally found, usually in chronic paranoid psychotic epileptics with bilateral independent EEG spike foci in the temporal lobes.

POST-OPERATIVE STUDY

General

The duration of the post-operative study in this group has been from 2 to 5 years. The patients have repeatedly re-attended hospital for clinical, psychometric and EEG study, or have been re-admitted for a few days or a week or two as need arose. Some have been visited in their own homes. Reports of all those concerned with the patient—parents, marital partners, employers, social workers, occupational therapists, nurses, etc., have been obtained.

Apart from common and perhaps specific effects of the operation which will be described, it was important in determining the effects of the operation to assess to what degree the psychiatric disturbances caused disability. For this purpose a rating scale was constructed, comparing the pre-operative and post-operative working abilities, sexuality, general psychiatric symptoms, aggressiveness and need for hospitalization, the highest total score in the scale being 10, the lowest 0. On this rating scale the post-operative scores showed gains (i.e. improvement) in 18 cases, were unchanged in 3 and showed losses in 5 cases. Gains of 10 points were recorded in 2 cases, of 8 points in two cases, and also of 6 points in two cases. Gains of 5, 4 and 3 points were recorded at each level for 3 patients. Two further patients gained 2 points and one patient 1 point. Three patients lost 1 point, one patient 4 points and one 6 points. Five patients were therefore worse on this assessment. All five patients continued to have seizures after operation. On the other hand improvement of scoring on the rating scale was witnessed in a few patients who continued to have occasional seizures post-operatively. However, this rating scale does not do justice to the reality of the improvement in those patients who had been in institutions, sometimes for years, prior to operation. There were 10 such cases. A return to normal life and to employment was witnessed in 6 of these patients.

Specific Changes

The specific changes observed in our patients' personalities, in some in marked degree, in others in much lesser degree, can be grouped under 5 headings. These are (1) changes in intellectual functions, (2) a reduction in aggressiveness, (3) a "turning in" of aggressiveness with a tendency to depressive mood, (4) changes in sexuality, and (5) an increased warmth in social relationships. These will now be considered.

1. *Changes in Intellectual Function.* These have already been reported by our psychologist colleagues, Meyer and Yates (1955). In agreement with previous workers they find no changes in general intellectual ability as measured by tests of I.Q., no change in memory or comprehension, but a specific loss of *learning* ability by auditory means has occurred in some degree, and sometimes in severe degree, in the 19 patients whose operations have been performed upon the dominant hemisphere. In manual workers this is little disability but in the professional classes and in those whose work involves the necessity for retaining information acquired by hearing others speaking, the disability may prove

crippling. One patient, for example, previously working as a telephonist, was forced post-operatively to abandon this occupation. Another patient, a furniture packer, gave up this occupation because he could not follow verbal instructions.

2. *Reduction in Aggressiveness.* This change, which recalls the "taming" seen in Klüver and Bucy's (1937) monkeys subjected to bilateral temporal lobe ablations, was a prominent feature in 12 patients who previously had been markedly aggressive characters. (Cases 4, 5, 6, 10, 13, 14, 19, 23, 24, 27, 30 and 31.) This "taming" can develop at any time after the operation and may or may not be permanent. This change may be observed by the patient himself or his relatives but was uniformly reported by all those in the hospital who were concerned with the care and rehabilitation of the patients. Relatives noticed that whereas before operation they had to be careful what they said to the patients and how they said it, post-operatively it was possible to talk easily to them without provoking an attack of irritability or impulsive anger. In these patients the tolerance of frustration appears to have increased, and at the same time the patients show insight over their changed reactivity to stressful events.

Examples:

Case 5. This was a 15-year-old girl whose aggression took many violent forms (see Falconer *et al.*, 1953). Immediately after the operation the parents remarked that they could talk with her quite easily without immediately provoking a disturbance in behaviour. This was seen in the first and second post-operative week and the patient herself said that she could remember "upsetting nurses" but no longer had the inclination to do so. Her parents are not well suited to each other and for many years they had expressed their own difficulties by arguing about the patient, and when this path was no longer available their marriage became more disorganized, but they still tended to use the patient as a battle ground.

Case 6. This patient, a 45-year-old man who had been living at home, was noted by his mother to be irrationally irritable at times. After a right temporal lobectomy he has become quite docile in his relationship to her even though his general psychiatric picture has not been noticeably improved.

3. *"Turning-in" of Aggressiveness with Depressive Mood.* In some patients in whom a reduction in outwardly turned aggressiveness was apparent, depressive mood swings, with or without hypochondriasis developed. These occurred in 11 patients (Cases 5, 6, 7, 15, 21, 22, 23, 24, 25, 26, 29 and 31), and in several the depression was so severe that re-admission to hospital became necessary because of incapacity, distress and suicidal risk. E.C.T. was given to 5 patients, (3-4 treatments) and resulted in a rapid recovery in 3 of them. These depressive states tended to recur up to 18 months after operation, had a tendency to spontaneous recovery and when necessary could usually be relieved by E.C.T. Such illnesses were not necessarily related to post-operative fits, which, except in one patient (Case 7), were not a serious issue. These cases are of particular interest from the psychopathological point of view, since during recovery from the depression, a swing over to paranoid hostile attitudes was occasionally witnessed. The relation between inwardly turned aggressiveness and the depressive mental state, and outwardly turned aggressiveness and the hostile paranoid state is apparent in such patients. The post-operative personality changes of the temporal lobectomized patient would seem to create a "model" psychosis susceptible of study because of its short duration, its intensity and its tendency to be altered by therapeutic measures.

Examples:

Case 7. This patient became extremely depressed towards the end of his first post-operative year. Before this he had shown mixed psychoneurotic symptoms. When the depression was

at its height it manifested itself more in hysterical conversion symptoms and behaviour, but the underlying feeling tone was distinctly that of an affective disorder. He was given electroconvulsive therapy without benefit, and as time went on this depression seemed to resolve itself into a paranoid state which finally required certification. Of the 27 cases his fit pattern was the least "temporal lobe" in character, and his frequent grand mal fits and generalized myoclonic epilepsy remain.

Case 15. This patient had already been in hospital for a depressive illness on several occasions pre-operatively and had been given E.C.T. previously with good but only short term effect. Post-operatively she fluctuated between depression and euphoria, and after a long bout of anorexia caused by cholecystitis she was admitted and again given E.C.T. with the same transient good effect.

Case 22. The pre-operative state of this patient showed an immature hysterical character disorder with swings between over-dramatized depression and elation. Post-operatively she was euphoric, but towards the end of her stay in hospital at the 10th post-operative month she became mildly depressed, and this has continued until the last few months when her affective state has become quite normal, and she is without any hysterical personality disabilities.

Case 25. This patient was mildly paranoid prior to the operation and remained quite unchanged until her discharge in the 8th post-operative week. From that time she became apathetic, seldom spoke, and frequently cried. She stated that she felt hopeless but was unable to give any depressive thought content. Punctuating this chronic state were aggressive outbursts of a more florid paranoid type when she accused her mother and sister of talking about her in some derogatory way. She was unwilling to enlarge on her ideas of reference. She was admitted in an acutely depressed state 11 months after operation but this fluctuated very rapidly; one day she would be crying, aggressive and paranoid, but the next day she would be agreeable, co-operative and apparently symptom free. The depressive picture predominated and she was given three E.C.T. which coincided with the disappearance of the depressive picture and an increase in the paranoid picture. She became so aggressive that this course of shock treatment was discontinued and by the time of her discharge 3 months later she showed neither depressive nor paranoid symptoms. Her recovery had persisted 5 months later.

Case 26. Electric shock treatment was administered to this patient for a picture that looked entirely depressive but later more paranoid.

Case 31. This patient before right temporal lobectomy showed a hysterical character liable to depression. A short course of E.C.T. brought about a rapid improvement in a severe depressive-hypochondriacal state which had been present after three post-operative weeks of euphoria. In the 9th post-operative month the depression necessitated admission, and a course of 5 electric convulsive treatments resulted in a clearing of the depression and an ability to express pent-up feelings. In the 8 months following her discharge aggressive feelings have disappeared, as has the depression. She appears now to be very much more mature.

4. Changes in Sexuality. In 11 patients changes in sexuality could not be assessed because the patients were either pubescent or menopausal. In Case 4 information was not available because of the patient's bland denials when interrogated. Evidence was however available in 16 cases (Cases 7, 8, 9, 10, 11, 12, 15, 18, 19, 21, 23, 24, 25, 26, 29 and 31). In two patients no change could be elicited (Cases 11 and 29). A change in the intensity of libido or in its direction and choice of object was noticed in 14 patients. One patient became impotent, but in general the change experienced was of increased sexual drive and potency. One patient became hypersexual and perverse, masturbating openly in the ward as many as 20 times a day, exhibiting himself to nurses and patients and making homosexual advances. This behaviour ceased when oestrin therapy was exhibited in large doses and did not recur when treatment was discontinued. In three patients (Cases 12, 13 and 24), two of whom have already been reported (Mitchell, Falconer and Hill, 1954; Pond and Bidwell, 1954) a loss of perverse sexual tendencies and a normal libidinal interest and activity occurred. One patient, who pre-operatively had shown sexual promiscuity and infidelity to his wife, developed a normal attitude to marriage.

5. Increased Warmth in Social Relationships. A striking change, particularly in the first post-operative months, is the increasing warmth in social

relations exhibited by this group of patients and detectable even in those, who on total assessment have not been significantly benefited by the operation. This increased warmth, or friendliness, is experienced by the observer as an increase of extroverted attitudes and is associated particularly with the reduced outwardly turned aggressiveness. The patients appear to be more concerned with the attitudes and feelings of others and in this sense can be said to show a lessening of the egotism which was a feature of the pre-operative personality.

In a few cases it has proved impossible to assess whether the personality has undergone improvement, for the change might result in a more acceptable social behaviour but much less acceptable effect on the patient.

Case 23. This patient considers that the operation was a total failure because of its effect on his attitude to work; he placed little value on the improvement which his wife and his doctor have noticed. He has very few fits, is a much more pleasant person to live with, and his work record has not actually suffered.

There are two cases whose personality and whose social adaptation have been affected for the worse by operation (Cases 7 and 26). Reference has been made to Case 7 above. Case 26 was a man who came from a disturbed family environment with a positive family history of epilepsy. He had suffered from temporal lobe epilepsy since the age of 15. The aura of "parasitic word" had undergone a slow change from "diploma" to "pluma" and then to a vague rather pleasurable cephalic rhythm. The fits tended to occur when his wife was menstruating and he had a group of serial grand mal fits after his wife aborted a six-month pregnancy. He had a clear spike focus in the left anterior temporal lobe and a left anterior temporal lobectomy was carried out in January, 1954. He remained three weeks in the Neurosurgical Unit during which time he had one focal fit implicating the left motor cortex. At this time he was noted to be markedly dysphasic when shown 10 fairly simple objects to name. In the 4th post-operative week he was able to name only four of them; he demonstrated the use of three and he would not attempt the remainder. His stay in a psychiatric ward lasted $4\frac{1}{2}$ weeks during which he was constantly requesting our agreement to his discharge. He showed great intolerance to noise of any sort and completely rejected any form of occupational therapy. Following his discharge late in February, 1954, he managed to return to his work as telephonist and to a marriage which had always been unstable. His wife noted that when at home there was a pronounced need to talk, mostly about himself, the treatment or the hospital. He was impossible to "contact" and seemed remote and disinterested in anyone but himself. He was rude and inconsiderate to others which was in marked contrast to his pre-operative personality. The egocentricity has remained unchanged since, although the pressure behind his talk has decreased. Because of his aphasia he was frequently at a loss to name or describe an event or a wish and his demands on his unintelligent wife to fill this gap, to tell him how to spell a word, were very excessive. This, his rudeness, his egocentricity and his impotence shook the already unsteady foundations of the marriage and they have obtained a legal separation. He remains almost fit-free, but is apathetic and because of his inability to learn (he calls it "memory") and his difficulty in speech he has had to be removed from his job as a telephonist. He is at present moderately paranoid in attitude. He says that we did the operation to remove the word "diploma" but failed because he can still remember it; he feels that one of us (W.M.) has implanted the word "insanity" in his mind and when he goes to bed this runs through his mind obsessively. Each time he is seen he brings up these two points and no amount of explanation alters them. He expresses them with frank hostility.

Post-operative Fits and Psychological Improvement

At the time of the present study, of the 27 patients, 11 were fit-free, 14 had only occasional seizures and showed great improvement in this respect, one had fewer seizures than before operation, and one was unchanged in this respect by the operation. This patient (Case 7) has already been described and is now a certified patient in a mental hospital. On the rating scale he lost 6 points and from the point of view of psychological change following the operation is the worst result. Another patient (Case 14) has had fewer fits since operation and shows slight psychological improvement, gaining two points on the rating scale. It is clear, however, that within the group of 25 patients who are either fit-free (11 cases) or greatly improved (14 cases), those who become fit-free show more consistent and greater psychological improvement than those who still suffer occasional seizures. The rating scales illustrate this. Of 11 fit-free patients six gained five points or more (one 10 points, two 8 points, one 6 points and two 5 points). Only one patient showed a loss of 1 point (Case 8). This patient died in status epilepticus after being fit-free for 2 years. By contrast, of 14 patients who still suffered occasional seizures only three patients gained 5 points or more on the rating scale (one 10 points, one 6 points and one 5 points). Two patients were unchanged in their scores and three patients showed losses (two of 1 point and one 4 points).

DISCUSSION

The success of the operation of temporal lobectomy for the relief of epileptic seizures may be expected to depend upon the surgeon's ability to remove those brain areas which are the sites of the primary epileptic discharge. This is most likely to occur when the pathological lesion is a discrete one, e.g. tumour or atrophic process confined within the anterior third of one temporal lobe, and where there is no evidence from EEG studies of firing areas outside the anticipated operation site and particularly on the opposite hemisphere. The total disability or incapacity of the patient who is the sufferer from temporal lobe epilepsy is not, however, due only to the occurrence of the seizures. In fact the seizures themselves may become of minor significance as a cause of disability, which is predominantly due to the serious changes of personality that develop. Disturbances of aggressiveness, intolerance of frustration, impulsiveness, changes in sexuality, egotism and mood disorder are then of greater significance than the seizures themselves.

In many patients in whom the personality disorder was severe, there was good evidence from the history and investigations that the patients had suffered *diffuse* brain damage. In these cases the temporal lobe lesions were but a part of the total pathological process. Nevertheless there is still evidence that the temporal lobe disorder itself contributes a significant specific effect upon the psychological functioning of the individual. This was seen particularly in those patients with personality disorder in whom discrete focal lesions were subsequently found in the temporal lobe removed and who subsequently had no further fits and showed a recovery from the personality disorder. The functions which would appear to us to relate most particularly to temporal lobe function are firstly, the ways in which aggressiveness is handled by the patients, secondly in the intensity and direction of libido, and thirdly, in the control over mood stability. Fourthly, there are also the specific functions related to cognitive ability, such as the learning ability.

It is not our purpose here to try and relate these functions to specific parts

of the temporal lobe. Suffice it to say that lesions of the deeper temporal structures particularly (e.g. uncus, hippocampal gyrus, hippocampus, and amygdaloid nucleus), both experimentally and clinically, may appear to provoke epilepsy on the one hand, and on the other may interfere with the regulating mechanism by which the impulse life of the individual is handled. This is seen in the intensity, direction and fluctuation of the sexual drive and in the expression and control of the aggressive response to frustration—both fundamental activities of the individual which the social conditioning process alters to the ethical demands of the environment. The effects of such lesions are greatest when the lesions occur in a brain still undergoing the processes of growth and maturation, i.e. in young patients, and are greater still when the social environment is disordered and the educational example of the parents itself inadequate. This was seen in the majority of our patients.

SUMMARY

1. A group of 27 patients, subjected to temporal lobectomy for epilepsy, have been studied post-operatively from periods varying from two to five years. The pre-operative and post-operative psychological status have been compared.

2. Pre-operatively 26 of the 27 patients showed some degree of psychological abnormality, which in many was very severe. Post-operatively improvement in some degree, but in some in very marked degree, was found in 18 patients.

3. Changes in learning ability, in aggressiveness, in sexuality and in social relationships are described.

4. The development of depressive illnesses was witnessed during the first post-operative two years in 11 patients.

5. A relationship is found between improvement in psychological status and post-operative freedom from seizures.

6. Assessed on a rating scale the psychological status of five patients was worse than before operation. All five patients continued to suffer epileptic seizures.

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Number	Sex	Age	Duration	Family		Early Environment	Pre-operative Personality	Mental Hospital	Side of Operation	Pre- and Post-operative Rating Scales					Gains or Losses	Depression Post-op.		Neuropathological Lesions		
				Epileptic	Mental					Aggression	Work & School	Sexuality	Psychiatric Symptoms	Hospitalization		Pre-operative	Dep.		Ect.	
4	M	32	30			Alc.	Character	—	D.	0/2	0/2	?	0/1	1/1	1/6	5	—	0	Non-specific changes	
5	F	15	7			Dis.	Character	—	N.D.	0/2	0/2	—	0/1	0/2	0/8	+8	—	0	Haemangioma calcificans	
6	M	45	43			Dep.	Paranoid	—	N.D.	0/1	1/2	0/0	0/1	1/1	3/5	+2	—	0	Sclerosis of Ammon's Horn	
7	M	45	27			Dis.	Character	—	N.D.	1/0	0/0	2/0	1/0	1/0	6/0	-6	—	+	Non-specific changes	
8	M	22	20			—	Character	—	N.D.	0/0	0/0	0/0	0/0	2/2	2/1	-1	—	0	Sclerosis of Ammon's Horn	
9	M	23	2			—	Character	—	D.	2/2	1/2	2/1	2/2	2/2	6/10	+4	—	0	Meningioma	
10	M	49	10			—	Character	—	D.	1/0	1/0	1/1	1/1	1/2	5/4	-1	—	+	Non-specific changes	
11	F	43	12			—	Paranoid	—	D.	1/1	2/2	0/2	0/2	1/2	4/9	+5	—	+	Capillary angioma	
12	M	38	30			?	Character	—	D.	0/2	0/2	0/2	0/2	0/2	0/10	+10	—	0	Non-specific changes	
13	M	13	10			Rei.	Character	—	D.	0/1	0/0	—	0/2	0/1	1/1	—	—	0	Sclerosis of Ammon's Horn	
14	F	21	19			Inst.	Character	—	D.	0/2	0/0	—	0/2	0/1	1/1	—	—	+	Non-specific changes	
15	F	25	14			Sep.	Depressive	—	D.	0/0	0/0	0/1	0/1	1/1	1/5	+4	—	0	Sclerosis of Ammon's Horn	
16	M	23	21			Dis.	Normal	—	N.D.	2/2	2/2	2/2	2/2	2/2	10/10	—	—	+	Sclerosis of Ammon's Horn	
17	M	47	17			—	Paranoid	—	N.D.	1/1	0/0	0/1	1/1	1/0	3/3	—	—	+	Cleatrix	
18	F	40	22			—	Character	—	D.	0/1	1/2	0/1	0/1	1/2	2/8	+6	—	+	Angioma	
19	F	48	1			—	Character	—	D.	—	1/1	—	—	2/2	—	—	—	+	Small infarct	
20	F	22	20			Pov.	Psychosis	—	D.	1/2	0/2	1/2	1/2	0/2	2/10	+8	—	0	Sclerosis of Ammon's Horn	
21	F	16	7			—	Character	—	N.D.	1/2	0/2	1/1	1/2	0/2	3/9	+6	—	+	Sclerosis of Ammon's Horn	
22	F	36	34			Sep.	Character	—	N.D.	1/1	2/2	1/2	1/1	1/1	6/7	+1	—	+	Sclerosis of Ammon's Horn	
23	F	23	22			—	Character	—	D.	0/2	0/2	0/2	0/2	0/2	0/10	+10	—	+	Sclerosis of Ammon's Horn	
24	F	20	22			—	Paranoid	—	D.	0/2	0/2	0/2	0/1	1/1	2/6	+4	—	+	Sclerosis of Ammon's Horn	
25	M	32	15			Alc.	Character	—	D.	1/0	2/2	2/1	0/1	1/0	1/1	7/3	-4	—	+	Non-specific changes
26	M	32	16			Dis.	Character	—	D.	0/1	0/2	—	0/2	1/2	3/6	+3	—	+	Non-specific changes	
27	M	45	42			Alc.	Depressive	—	D.	1/2	0/1	—	1/2	1/2	3/8	+5	—	+	Oligodendroglioma	
28	M	56	6			?	Paranoid	—	D.	1/1	0/1	1/0	1/0	1/1	4/3	-1	—	+	Non-specific changes	
29	M	42	4			—	Character	—	N.D.	1/2	1/2	1/1	1/2	2/2	6/9	+3	—	0	Oligodendroglioma	
30	F	51	11			Dis.	Character	—	N.D.	1/2	1/2	1/2	1/2	2/1	2/1	+3	—	+	Sclerosis of Ammon's Horn	
31	F	27	12			—	Character	—	N.D.	1/2	1/2	1/2	1/2	2/1	2/1	—	—	+	Sclerosis of Ammon's Horn	
Early Environment:																		(10 total maximum).		
Alc. = alcoholic parent.																		0	=	notoriously frequent outbursts.
Dis. = parental discord.																		1	=	occasional tractable outbursts.
Dep. = maternal deprivation.																		2	=	no outbursts.
Rej. = maternal rejection.																				
Inst. = institutional upbringing.																				
Sep. = parents separated.																		0	=	no work or very little.
Pov. = poverty.																		1	=	part-time work.
																		2	=	full work.
Pre-operative Personality:																				
Character = character disorder.																		0	=	perverse.
Paranoid = paranoid psychosis																		1	=	impotent, frigid.
																		2	=	normal.
Side of Operation:																				
D. = dominant hemisphere.																				
N.D. = non-dominant hemisphere.																				
Post-operative Fits:																		0	=	significant symptoms in complaint.
																		1	=	symptoms present but not prominent.
																		2	=	no symptoms.
																		0	=	frequent admissions or continued stay.
																		1	=	occasional and brief admission.
																		2	=	none.

ABILITIES AND BEHAVIOUR OF EPILEPTIC CHILDREN

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A SURVEY of the literature on handicapped children reveals comparatively little on the subject of epilepsy.

Most of the articles dealing with cognitive functions have been concerned wholly or mainly with verbal intelligence as measured by tests of the "Binet" type. Those relating to behavioural or personality traits have been mainly impressionistic.

Jones (1953), in a review of research in epilepsy, stated that there were few valid results in the literature, due, probably, to the inadequacy of research techniques.

Folsom (1953), reviewing recent investigations into the cognitive function in epilepsy, said that "Clinicians, studying epileptics in institutions, have reported cognitive deficit as an almost universal phenomenon in epilepsy . . . with increasing intensive study of non-institutionalized epileptic groups it has been clear that the I.Q. level of epileptics varies over the same range as that of the non-epileptic population, that whatever deficit may be characteristic of the epileptic, it is not a low I.Q." She stated that the deficits described were considered to be:

1. The effect of seizures.
2. The expression of an epileptic constitution or character.
3. Secondary to an affective disorder.
4. Together with seizures, a psychological reaction to the environment, with a typical character make-up as a predisposing factor.
5. A result of brain damage.
6. A result of psychosis.
7. Primary mental defect.

She pointed out that clinical evaluations are apt to be subjective; open to bias in the selection of cases, in the choice of processes to be investigated, and in the methods of investigation.

Folsom recommends (*inter alia*), that for future work, epileptics should be subdivided on the basis of cerebral dysfunction and seizure phenomena; that a battery of tests should be used to cover a range of intellectual processes; and that control groups should be more extensively used.

Pruyser (1953), dealing with the personality aspect of psychological testing in epilepsy, said that few psychologists had applied their methods to the study of the aetiology of personality disturbances in epileptics. He stated that the following propositions have been made in this sphere:

- (a) That the disturbances are directly associated with the pathological brain state which also produces the seizures.
- (b) That the epileptic personality syndrome is of constitutional origin, and as much an integral part of epilepsy as the seizure phenomena.
- (c) That the disturbances are reactive to psychosocial stress.

(d) That some epileptics, like anyone suffering from some disease, may react to their condition as if it were the dominant theme of their lives, to which everything else is made subservient.

Of the fifty references in Pruyser's article, only four studies dealt with the personality of epileptic children.

As a background to the present survey we cite some of the recent findings on abilities and behaviour of epileptic children.

COGNITIVE ABILITIES

1. *Verbal Intelligence*

Various estimates of the intelligence level of epileptic children have appeared from time to time, some based upon test results and some upon clinical impressions.

Table I summarizes test results of some recent investigations in this country and in America.

TABLE I
Intelligence Quotients Reported on Epileptic Children

Year	Source	Description	N.	Mean I.Q.
1947	Chalfont Colony	Special School Cases		66.0
1947	Lingfield Colony	Special School Cases		80.0
1947	Collins and Lennox	Clinic Cases	100	104.1
		(Petit Mal)	18	113.2
		(Psychomotor)	8	108.6
		(Grand Mal)	36	105.3
		(Grand Mal and Petit Mal) ..	25	102.7
		(Grand Mal and Psychomotor)	11	102.2
		(Brain Injured)		96.7
1951	Zimmerman <i>et al.</i>	Clinic Cases	100	93.5
		(Idiopathic Petit Mal)		105.5
		(Grand Mal and Idiopathic Petit Mal)		91.5
		(Grand Mal)		91.2
		(Symptomatic)		89.0
		(Traumatic)		89.0
1954	Henderson	Normal School Cases	365	95.5*
1955	Tenny	Special School Programme, Detroit	690	84.0

From the above results it would appear that the (verbal) intelligence level is related to the kind of school attended, and the type of epilepsy; with higher I.Q.s in normal schools, in idiopathic epilepsy and in petit mal.

Collins and Lennox (1946) found a negative correlation between intelligence and age of onset, i.e. a lower intelligence level with earlier onset.

They also reported a relationship between intelligence and electrographic patterns—the cases with mildly slow or fast waves had the higher I.Q.s, average 108, and those with very fast or very slow waves had the lower, average 92.4.

According to Folsom (1953), “Investigations to date reveal no consistent relationship between cognitive deficit, as evaluated by low I.Q. or decrease in I.Q. on retest, and the factors of duration of illness, severity or frequency of seizures, or absolute number of seizures . . . the wide variability in age groups in most of the investigations mentioned preclude a proper evaluation of these factors.”

* Median Score.

2. *Other Abilities*

In Burt's survey (1937), the epileptic children showed "typical slowness of mental reaction, and marked failures of memory and motor promptitude".

According to Bradley (1946), "The attention span is apt to be short and power of concentration poor. In school, arithmetic is most likely to be the subject presenting exceptional difficulty." In a later article (1947), he mentions "vacillations in ability to recall material which has been previously learned".

PERSONALITY AND BEHAVIOUR DISORDERS

In this sphere controversy revolves around a central issue, i.e. whether there is or is not an "Epileptic Personality". At one time few clinicians had any doubt in the matter. All epileptics were said to have well-marked and mostly undesirable characteristics. As mentioned before, impressions were based almost entirely upon experience with institutional cases. The full catalogue of epileptic traits mentioned in the literature is a lengthy one. Although there has not been complete agreement as to individual traits, some of them have been repeated so often that they have taken on the status of essential attributes.

Since the interest has moved to extra-mural epileptics there has been a swing of the pendulum, with a tendency to affirm that there is no such thing as an "Epileptic Personality". In the writer's experience, many workers say that "There is something about epileptics", but they would not like to commit themselves about specific traits. They might agree with Piotrowski (Pruyser, 1953), that "Epileptics share many traits but they cannot all be said to have the same personality".

Some writers believe that the "Epileptic Personality", if there is such, manifests itself gradually over a period of years, and that it is much less evident in epileptic children.

Some of the recent evidence for and against the existence of specific traits in epileptic children should, perhaps, be mentioned.

Bradley (1946) said, "The writer's experience with large numbers of ambulatory, intelligent convulsive children sharing with other patients the full and active life of a children's psychiatric hospital, free from community prejudice and parental worries, has convinced him that the personal characteristics and problems of most convulsive children are very similar to one another if one knows what to look for. Moreover, these characteristics distinguish most such patients from their non-convulsive brothers and sisters prior to entering the neutral environment in which they have been observed. Sudden variability in mood with impulsive actions and periodic, apparently unprecipitated irritability are notably conspicuous. Physical hyperactivity and restlessness are almost as prominent . . . No one of these symptoms is, of course, specific for an underlying convulsive disorder, but their combination is so frequent as to be impressive. They are also seen in children with electroencephalographic patterns of typical convulsive form even when the seizures are of rare occurrence or have apparently never been observed clinically. This suggests that repeated disturbance of the brain physiology which we can record electrically even though we do not understand it provides an 'organic' or 'constitutional' basis for the behaviour of such children regardless of their other life experience." Bradley also recognizes secondary reactions to the convulsive disorders and the resulting distorted attitudes of the world towards them (1947). He mentions, for example, feelings of rejection, hostility, aggressiveness, incessant demand for attention, delinquency, sense of guilt.

Bridge (1949) spoke of prodromal personality disorders. There was, he thought, a physical basis for the early symptoms, but there was probably exacerbation due to environmental causes. He also stated that the behaviour of epileptic children was related to the previous personality and gave lists of traits which he found in aggressive and in submissive individuals. He was, however, of the opinion that there was no basis for the concept of the "Epileptic Personality".

Pond (1942) said that *petit mal* children were usually stubborn, passive, and serious, while brain-damaged children were aggressive.

Some writers have found signs characteristic of epileptic children in the Rorschach Test which, as Pruyser says (1953), appears to have been the chief tool used by psychologists in the investigation of the epileptic's personality.

Delay *et al.* (1955), in their excellent review of the use of the Rorschach Test in epilepsy, mention four *controlled* studies on epileptic children, by Kogan (1947), Arluck (1940), Zehrer (1951), and Franco (1952). These studies tried to answer the following questions:

- (a) Is the personality of the epileptic child different from that of the normal child?
- (b) Does the personality of the epileptic child differ from that of non-epileptic children with behaviour disorders ('troubles du caractère')?
- (c) What is the role of reaction to the illness in epileptic children; and
- (d) What part does heredity play in the epileptic personality?

None of these studies is decisive, and the authors (Delay *et al.*) conclude (p. 146): "Il est à l'heure actuelle impossible de considérer comme acquis aucun résultat concernant le test de Rorschach chez les enfants épileptiques. Les rares études contrôlées consacrées à ce problème donnent des résultats contradictoires. S'il est probable que les mêmes facteurs jouent dans la formation de la personnalité des enfants épileptiques que dans celle des adultes, la démonstration définitive reste encore à apporter."

Many writers have found the behaviour of epileptic children to be similar to that of brain-injured children, e.g. Pacella (1948), Bender (Bradley, 1951). Some consider that the behaviour is due, wholly or partly, to cerebral dysrhythmia. Thus, Sands (1946)—"Some of the behaviour in a person afflicted with epilepsy seems to be due to disturbances in the chemical and electrical reactions in the brain cells, for often, with treatment by drugs, some behaviour symptoms disappear immediately."* He goes on to say "The personality of a child with epilepsy is not the result of his seizures. On the contrary, his personality, like that of any other child, will or will not be wholesome, depending upon how his parents, family, playmates, teachers, and others accept him as an individual."

Jasper (Bradley, 1951) said that "any part of the brain occupied by epileptiform discharge is lost to the individual for normal adaptation or integrative behaviour".

It is, of course, common knowledge that the behaviour of some epileptic children is somewhat worse in the pre-ictal period and better in the post-ictal period. In some others the reverse has been reported, though malbehaviour soon after an attack may sometimes be due to mismanagement of the patient during a confusional phase.

The accumulated evidence for and against an "Epileptic Personality" is scientifically unconvincing. It is questionable whether tests of the projection

* Price (1950) points out that toxicity produced by overdosage can produce behaviour disorders.

type will add much to our knowledge on this subject. The writer agrees with Pruyser (1953) that "If one wants to study the psychodynamic correlates of paroxysmal symptoms, such as epilepsy, a longitudinal approach seems to be indicated. The patient should be studied from day to day, or from hour to hour, and the focus of the study should be the changes that take place in the important psychological variables.

In the present study we have indicated some variables which might usefully be taken into account in such a study. Our findings do not support the concept of an "Epileptic Personality" in children; rather they stress the complexity of the problem, and the unwisdom of adding further purely impressionistic evidence to an already shaky edifice.

THE PRESENT INVESTIGATION

The general aim of the investigation was to verify or otherwise some of the traits attributed to epileptic children and, in particular:

(i) To study certain cognitive abilities of epileptic children drawn from three kinds of schools—normal schools, a special residential school for epileptic children, and a school for the physically-handicapped.

(ii) To compare the test results of the epileptic children with those of a group of non-epileptic children, individually matched as closely as possible for age and sex with the epileptic group.

(iii) To correlate test scores with certain variables associated with epilepsy, and

(iv) To correlate these variables with each other.

An ancillary study of behaviour at home and at school has been made, based upon information received from parents, teachers, doctors, and others who have had dealings with the children.

The Epileptic Subjects

The main group consisted of fifty-six epileptic children equally divided as to sex—twenty-eight from normal schools in Birmingham, and twenty-eight Birmingham children attending a special residential school for epileptics. The secondary group consisted of twelve epileptic children at a physically-handicapped school in Birmingham; most of these children had hemiplegia or quadriplegia in addition to their epilepsy.

The composition of the sample was, therefore: normal schools 41 per cent., special school for epileptics 41 per cent., and physically-handicapped school 18 per cent.

This compares with a distribution of epileptic children in Birmingham of: normal schools 61 per cent., residential school 23 per cent., physically-handicapped schools 6 per cent., educationally subnormal schools 7 per cent., others 3 per cent. (1953). The median ages of the main sample were: girls 11·0 years, boys 11·5 years.

The Control Group

This was drawn from primary and secondary schools in an "average" district. The children were matched, as nearly as possible, with the epileptic children for age—girls and boys separately and individually. The socio-economic level was approximately the same for both epileptic and control groups. The original intention was to take a random sample at each age from the school registers, but as the schools were streamed it was decided to aim instead at the

same proportion of A, B, and C-stream children in the sample as in the school population, and this was done at each age-level. Two of the cases had to be excluded later, and the control group therefore comprises 54 cases as against the 56 epileptic cases. Girls and boys are equal in number. The median age was 10·5 years in each case.

The median I.Q. of the control group is 100·5.

The Tests

A large number of tests was considered; the following being finally selected:

1. The Revised Stanford-Binet Tests of Intelligence (Terman-Merrill Scale, Form L.) 1937.
2. The Gates' Oral Paragraph Reading Test (part of the Gates' Diagnostic Reading Tests), 1945.
3. The Burt Oral (Graded) Arithmetical Problems Test, 1939.
4. The Van der Lugt Psychomotor Series, Tests 1, 6, 9, 10.

As the last-named series is not much used in this country a brief description of the tests follows:

The series tests, primarily, manual ability—"continuous control of one or both hands and fingers". Speed, pressure, accuracy, co-ordination, steadiness, and motor memory are involved. The norms were derived from 2,128 Dutch children and 700 American children adjusted to the original sample. The four tests used here—three of them timed—were selected because they are almost "pure" dexterity tests, i.e. little intellectual effort is required. Mental ages are available for each test.

Test 1 has a rectangular metal base, a rod to screw into it, and fifteen metal rings. The rings are removed from the rod, one at a time, and placed on the table as quickly as possible with each hand separately. The rod is then unscrewed and the rings are carried singly across the baseboard and back again with the board in two positions. Finally, they are moved right round the board with each hand.

Test 6 consists of a metal bar as pencil holder, with a central thumbscrew. A pencil is fixed in the bar to project five inches. The subject holds the bar with the first three fingers and thumb of both hands and tries to draw straight lines eight inches long from top to bottom of the paper and vice versa. The lines are drawn with and without guide lines.

Test 9 has a rectangular metal base drilled with thirty-two holes, sixteen with left-hand thread and sixteen with right-hand. At each end of the base is a container with the appropriate screws. The subject takes up a screw with each hand and screws them into the holes simultaneously, until the board is full.

Test 10 has a square metal base with eighteen "cups" and eighteen smaller holes in it, alternately. There are two containers, one to the left of the board with marbles in it, and the other to the right with metal pegs in it. The subject has to pick up a marble and a peg and place them simultaneously in the receptacles. For alternate placings the hands have to be crossed.

RESULTS OF THE SURVEY

1. Composite Test Results

Table II shows the mean quotients for the epileptic and control groups on the four tests separately and on the average of the four.

TABLE II
Mean Scores on the Four Tests and on the Average of the Four: Epileptics and Controls

Group					Mean Test Scores				
					Intelligence	Reading	Arithmetic	Motor	Average
Controls:									
All	..	..	..	..	99·8	102·9	83·0	100·4	96·3
Girls	..	..	..	..	96·1	102·3	80·0	96·3	93·9
Boys	..	..	..	..	103·4	103·4	85·0	103·7	98·4
Epileptics:									
All	..	..	..	..	87·5	94·0	75·3	88·2	86·3
Girls	..	..	..	..	83·0	91·7	71·2	83·9	82·3
Boys	..	..	..	..	92·1	96·4	79·4	92·5	90·3
Normal schools	..	..	..	..	95·9	100·6	83·3	91·5	92·9
Residential school	..	..	..	..	79·2	83·5	66·6	84·9	79·7
P.H. school (brain injured)	..	..	..	..	60·0	71·5	51·0	51·7	58·4
(Brain injured, main sample)	..	..	..	..	78·0	81·5	63·1	71·0	72·8

The composite score-deficit of the epileptic children in the main sample, when compared with controls, is 10 points, or, approximately nine months of mental age.

Deficits on individual tests range from 7·7 points on mental arithmetic to 12·3 points on verbal intelligence and psychomotor tests.

As expected, the normal schools epileptics score higher than the residential school cases on all four tests, the average (composite) discrepancy between them being 13·2 points, with individual test-differences ranging from 6·6 points on the psychomotor series to approximately 17 points on the other three tests. At the extremes are the normal school boys, whose scores are almost on a par with the control group scores (on the arithmetic test they are actually superior to the control group by 5 points) and the residential school girls, whose average deficit is 17·4 points.

Boys are superior to girls in both epileptic and control groups, the superiority being only slightly greater in the epileptic group, i.e. 8 points as against 4·5 points in the control group. The epileptic boys have a slightly higher "scatter" than the epileptic girls on all tests except the psychomotor series.

The significance of the score-differences between the epileptic and control groups was determined by the critical ratio method (Diff./S.E. Diff.). With the following exceptions, all differences are significant at or above the 5 per cent. level of confidence:

Epileptic boys—reading and arithmetic.

Normal schools epileptics—intelligence, reading, arithmetic.

The merit-order of the tests is the same for both epileptic and control groups, with reading highest and arithmetic lowest, and the range of scores between these two tests is almost identical for the two groups, i.e. approximately 20 points.

On the other hand, the "scatter" or intra-test score variation as measured by V-scores (100 S.D./M) is approximately 60 per cent. greater in the main epileptic group than in the control group.

In general, therefore, the epileptic children score lower on all four tests, though the differences are not very great. The general pattern of abilities is the same for epileptics and controls, but the epileptic children are more heterogeneous in their scores. The range of scores in the epileptic group is from 38 to 145 points and in the control group 57 to 141 points.

2. The Tests Considered Individually

(a) *Intelligence Test.* This test shares with the psychomotor series the highest score-deficit in the main epileptic group, i.e. 12.3 points. The test shows the least scatter of the four.

Collins and Lennox (1946), analysed items of the Terman-Merrill Scale on 100 epileptic children, placing the items in order of merit. No control group was used in their survey. The highest scores were on Orientation, Numerical Items, and Verbal Items, in that order. Memory, Judgment and Reasoning items came next, with Form-Perception lowest.

We have analysed the Terman-Merrill results, using different categories from those of Collins and Lennox. Table III shows the score-discrepancies of the epileptic children as compared with controls.

TABLE III
Mean Score-Discrepancies, Epileptics and Controls, on Five Categories of the Terman-Merrill Scale (Mental Ages)

Group	Word Associ- ation	Mem- ory	Lan- guage	Reason- ing	Per- ception	Average
All epileptics	-2.81	-2.28	-1.85	-1.61	-1.62	-1.91
Epileptic girls	-2.95	-2.40	-1.83	-1.63	-1.65	-1.95
Normal schools epileptics ..	-2.08	-2.13	-1.86	-1.57	-0.94	-1.85
Residential epileptics ..	-3.79	-3.61	-3.28	-2.99	-2.81	-3.26

As the Table shows, Word Association, i.e. saying as many words as possible in one minute, shows the greatest discrepancy. The epileptic children produced, on the average, the same number of words as the control group, i.e. 25, but the average age of the epileptics to whom the items were administered was over two years higher than that of the corresponding control cases.

Immediate memory for various kinds of material, visual and auditory, shows the next highest deficit. This would seem to support Burt's findings (1937) of "marked failures of memory . . ." in epileptic children.

Linguistic items, e.g. vocabulary, rhymes, sentence completion, disordered sentences, etc., follow, with a deficit of 1.8 years.

Reasoning, e.g. giving opposites, and similarities, spotting absurdities, problem solving, etc., shares fourth place with perception, e.g. "Search", report, etc., with a deficit of 1.6 years.

In our survey, therefore, perception and reasoning show the highest scores, while in Collins and Lennox's survey they rank lowest. The two surveys agree, broadly, in regard to verbal and memory items.

(b) *The Reading Test.* This test produced the highest scores of the four in all the epileptic groups and in the control group. It also shows the highest "scatter" and is the most sensitive to the influence of other variables. There was no obvious difference between the two groups as to the type of reading errors committed.

(c) *The Arithmetic Test.* Our results confirm that epileptic children find difficulty with mental arithmetic in relation to other subjects, but the difficulty is not confined to them. The control group had similar difficulty, and, indeed, the epileptic deficit of 7.7 points on this test is the smallest of the four. The normal schools epileptic boys actually score 5 points higher than the control group on this test. Only three children out of 122 achieved the "ceiling" age on this test, i.e. 14+ years. The epileptic children with cerebral injury found exceptional difficulty with the test. Those in the main sample averaged only 63

points, while those in the physically-handicapped school averaged as low as 51 points.

It should be mentioned that this test is untimed, and it may be that on a timed test the epileptic deficit might have been greater, but we have no evidence to offer on this point. The author of the test says (1939) "There is no time element . . . care must be taken to give even the most tardy time to arrive at the best answer he can".

(d) *The Psychomotor Test*. The epileptic deficit of 12·2 points is almost identical with that on the verbal intelligence test.

The residential school cases did comparatively well on this series, their average score being only 6·6 points below that of the normal schools epileptics.

The brain-injured cases in the main sample also performed quite creditably on the test, with a mean score of 71 points—8 points higher than their arithmetic score. The children with physical handicaps, on the other hand, had the same mean score (51 points) on both these tests. They were, of course, handicapped on the manual tests by a loss of power in one or both hands.

Of the four subtests in the series, the greatest deficit of the epileptic group (14 points) was on the screws test, which requires no mean degree of dexterity. The smallest deficit of 5 points was on the two-handed drawing test, which is untimed.

The results of the psychomotor series on the epileptic group as a whole, support Burt's findings (1937) as to "failure of motor promptitude" in epileptic children.

3. Test Results Related to Other Variables

In Table IV we have shown test score discrepancies associated with certain variables—as compared with the rest of the cases, in each instance.

TABLE IV
Mean Score-Discrepancies (Quotients) for Certain Aspects of Epilepsy

Variable	Intelligence	Reading	Arithmetic	Motor	Composite
Grand mal seizures ..	+13·4	+11·3	+11·9	-0·9	+8·9
Positive family history ..	+4·7	-0·8	+8·7	+2·9	+3·9
Firstborn children ..	+3·5	+1·9	-3·7	-3·5	-0·4
Early neurosis ..	+1·0	+2·0	-2·0	-5·0	-1·0
Late milestones ..	-2·4	+1·0	-5·9	-1·9	-2·3
Petit mal seizures ..	-4·0	-12·7	-0·8	+5·5	-3·0
Frequent seizures ..	-10·0	+5·0	-9·0	+1·0	-3·2
Grossly abnormal EEG ..	-4·0	-7·3	-7·2	+5·4	-3·2
Abnormal birth ..	-0·3	-1·5	-2·9	-10·3	-3·7
Dual seizures ..	-9·3	+1·4	-11·1	-4·6	-5·9
Longer duration ..	-5·2	-9·0	-6·0	-12·5	-8·2
Earlier onset ..	-4·7	-11·5	-4·0	-15·7	-9·0
Precipitants ..	-8·0	-11·0	-8·0	-10·0	-9·2
Residential school ..	-16·7	-17·1	-16·7	-6·6	-14·3
Negative behaviour ..	-10·4	-21·6	-12·2	-14·8	-14·8
Brain injury: main sample	-11·1	-14·7	-14·5	-20·0	-15·1
Bad behaviour ..	-15·5	-28·1	-18·2	-15·5	-19·3
Brain injury: P.H. school	-30·8	-24·4	-27·0	-38·7	-30·2

It will be noted that the highest score-deficits (composite score) are associated with brain injury, bad behaviour, negative behaviour, and attendance at the residential school. An accumulation of such attributes would be likely to have serious adverse effects upon mental functions. Only two of the variables

are associated with higher scores, i.e. grand mal seizures and a family history of epilepsy. Only two groups in fact (not in the table) have higher scores, i.e. the normal schools cases and the good behaviour cases.

Variables such as primogeniture, early neurosis, late milestones, petit mal seizures, frequent seizures, abnormal birth, and grossly abnormal EEGs seem to affect scores (composite) comparatively little.

4. *Behaviour Disorders*

Information about the personality and behaviour of the epileptic children was obtained from parents, teachers, school medical officers, case files of the Birmingham Children's Hospital and others who had dealings with the children. The behaviour enquiry is supplementary to the main study of cognitive abilities. It was not intended to be a thorough and intensive investigation on the lines suggested by Pruyser (1953).

We were able to divide our main sample, on the basis of their behaviour, into three fairly well-defined groups, i.e. "Good", "Negative", and "Bad" behaviour groups, with frequencies of 37 per cent., 28 per cent., and 35 per cent. respectively. (Bridge found, among his 742 epileptic children, 9 per cent. with severe personality disorders, 37 per cent. with moderate disorders, and 54 per cent. "None or unknown".)

The badly-behaved children in our survey were described as "Aggressive, violent, bad-tempered, truculent, insolent, spiteful, destructive, sadistic, in need of constant supervision," etc. There were, in this group, eight girls and twelve boys.

The "negative" behaviour group—ten girls and five boys—were described as "sullen, solitary, low-spirited, easily led, timid, unstable, oversensitive, moody, immature, dependent," etc. These traits were, in general, less troublesome socially, but were none the less a bar to proper school progress.

The "good" cases, equally divided as to sex, showed few undesirable traits. Some were described in felicitous terms like "Polite, happy, lovable, pleasant, popular," etc.

The average (composite) test scores for the above three groups were 77.2 points, 81.6 points, and 96.5 points respectively, i.e. there was a definite downward trend in test scores with worsening behaviour.

Taking other variables, the only one which shows a significant correlation with bad behaviour is attendance at the residential school ($P = .01$). This is understandable, since bad behaviour is one of the reasons for referring epileptic children to the school.* Other correlations—or associations—with bad behaviour which, though not significant, showed definite trends, were brain injury, precipitating circumstances ("Symptomatic" cases), grossly abnormal EEGs, petit mal seizures, frequent seizures, late milestones, and longer duration of the epilepsy.

Negative behaviour shows no significant associations with other variables, and the only noteworthy associations are with primogeniture, and late milestones.

Good behaviour shows positive, though not significant associations with grand mal seizures, normal milestones, primogeniture, attendance at normal

* At the time of the survey thirteen of the 28 residential school cases had been referred there because of severity or frequency of seizures, six for bad behaviour, and nine for both seizures and behaviour. About 75 per cent. of the residential cases showed some undesirable traits (bad or negative behaviour) as against about 50 per cent. in the normal schools.

schools, and shorter duration. There are no known cases of brain injury among the well-behaved children.

When a comparison was made between the twelve "best" and the twelve "worst" cases in the main epileptic sample the differences mentioned were strongly accentuated. The difference between the two groups on test scores (composite) rose to 26 points.

It is evident that the behaviour of this group of epileptic children is associated with and perhaps consequent upon very many factors, some of which have been mentioned. Such variables would need to be taken into account in a scientific study of epileptic behaviour.

5. *Relationships Between Variables Other than Tests*

In all, eighteen variables were studied, and the degree of association between each one and all the others was determined by the χ^2 method. Seizure-type and behaviour are trichotomous variables, all the rest being dichotomous.

As Table V shows, five associations are significant at or above the 5 per cent. level of confidence, while a further eight are between the 5 per cent. and 10 per cent. levels of confidence. Other associations showed definite trends without being statistically significant.

TABLE V
Associations Between Certain Epileptic Variables

Variables	χ^2	Significance Level
Dual and frequent seizures	19.00	.01
Dual seizures and residential school	19.30	.01
Residential school and bad behaviour	8.44	.01
Positive history of epilepsy and early neurosis	5.72	.02
Dual seizures and normal birth	5.65	.02
Late milestones and early neurosis	3.59	.06
Precipitants and bad behaviour	5.60	.06
Negative family history and grossly abnormal EEG	3.35	.07
Abnormal birth and primogeniture	3.33	.08
Abnormal birth and late milestones	2.90	.09
Negative family history and primogeniture	2.62	.10
Male sex and positive family history	2.58	.10
Negative behaviour and primogeniture	4.61	.10

Behaviour has been mentioned in the previous section: the other variables and their associations are as follows:

Sex Differences

The epileptic girls have a higher incidence of negative behaviour, grand mal seizures, abnormal births, and grossly abnormal EEGs, while the boys are higher in respect of positive family history, bad behaviour, frequent seizures, and dual seizures. None of these differences is, however, statistically significant.

School

Epileptic children at the residential school have more cases of dual seizures ($P = .01$), bad behaviour ($P = .01$), grossly abnormal EEGs, frequent seizures, and precipitants; while those attending normal schools were higher in respect of abnormal births, grand mal seizures ($P = .01$), early neurosis, negative

behaviour, late milestones, positive family history, and earlier onset. There is no difference in regard to duration of epilepsy.

Price (1950) found that normal school epileptics were characterized by later onset and shorter duration, less frequent seizures, idiopathic epilepsy, and normal capacity. We find ourselves in agreement on the last three points but not on the first two.

Age of Onset and Duration

Both earlier onset and longer duration (based upon a comparison of two groups of 0 to 5 and 6 to 11 years in each instance) have an overall adverse effect, the latter slightly more so than the former.

Four variables are associated with both early onset and longer duration, viz. brain injury, precipitants, abnormal birth, and early neurosis.

Petit mal seizures and bad behaviour are associated with a longer duration, while grand mal seizures show a later onset and shorter duration—by two years.

Folsom (1953) suggests that early onset may have adverse effects, (i) by interfering with normal schooling; (ii) by the production of permanent changes in the immature brain by the grand mal seizures; and (iii) by early brain damage resulting in early "mental defect".

Type of Seizure

Of the fifty-six cases in our sample nineteen (34 per cent.) suffered from grand mal, twenty-one (37 per cent.) from petit mal, including akinetic seizures, and sixteen (29 per cent.) from dual seizures, i.e. grand mal and petit mal.* Our sample has a higher proportion of dual cases than is commonly reported. Most of them attended the residential school, where it is possible that seizures are more closely observed than elsewhere, apart from the likelihood that such cases were referred there because they were dual cases.

Two correlations (χ^2) are statistically significant at the 1 per cent. level of confidence, i.e. dual seizures with frequent seizures and residential school attendance. Other notable associations with dual seizures are abnormal birth and grossly abnormal EEGs. Grand mal seizures are associated mainly with abnormal birth, good behaviour, infrequent seizures, and normal school. The only association worthy of note with petit mal seizures is a much longer duration of epilepsy— $7\frac{8}{12}$ years.

Frequency of Seizures

Thirty-two per cent. of our cases had frequent seizures, and 57 per cent. infrequent seizures. The composite test results for the main sample showed only two points' difference between the cases with frequent and those with infrequent seizures. The frequent grand mal cases were 12 points lower than the infrequent cases, while the frequent petit mal cases actually had an 11-point superiority. The latter result is interesting in the light of Lennox's statement (1945) that personality or intelligence may be barely affected by thousands of petit mal seizures.

Precipitating Circumstances

In thirty-three (59 per cent.) of our cases the onset of epilepsy closely

* It is possible that a more intensive investigation of these cases in the light of recent typology, e.g. that of Penfield and Jasper (1954) may have yielded a different classification.

followed an event which we have called a precipitating circumstance—a term which is more commonly used in relation to individual seizures.

Fourteen cases of “Shock” formed the largest sub-group. One boy saw his father’s “leg blown off and eye blown out” in an air raid, two girls witnessed the death of near relatives, one girl was frightened by an Alsatian dog putting its paws on her shoulders, another girl’s fits started soon after seeing a “horror” film, and one boy’s after being pushed into a swimming bath.

The second largest sub-group—nine cases—was that of the febrile illnesses such as meningitis, pneumonia, rheumatic fever.

Other precipitants were accidents, physical shock, etc. These cases have an average score-deficit of 10 points, and a slightly positive overall association with other adverse variables (Table VI). The more notable associations were with bad behaviour, abnormal birth, and primogeniture. There was a negative association with grossly abnormal EEGs. The age of onset was much earlier in this group.

Cerebral Injury

Eight cases in the main sample—four of each sex—gave evidence of cerebral injury at birth or subsequently. Other cases might, by inference, have been included, but the evidence was not conclusive. This group of cases, though small, is an interesting one, and brief histories of four of them are appended.

The average test score of these cases—72 points—was 14 points below that of the rest of the sample, and 23 points below that of the controls. The brain-injured physically-handicapped cases have the lowest scores, average 58 points.

Family History of Epilepsy

There seems to be some disagreement as to the incidence of a family history of epilepsy. Some writers believe that a hereditary factor is present in most, if not in all cases of epilepsy, e.g. Shanks (1949). Alström (Fairfield, 1954) found no history of epilepsy in 889 out of 897 cases. Lennox (1951) reported a history of seizures in 20 per cent. of relatives, while Tenny (1955) found 43 per cent. positive history in 765 epileptic children. Kimball and Hersh (1955) in an analysis of 520 sibships in which at least one child was epileptic found 294 cases with one parent affected, 222 with neither, and 4 with both. They stated that if one parent has a genetic constitution for epilepsy, the frequency among children is about 36 per cent., and if one child in such a family has epilepsy, the numerical probability of another child having epilepsy is about one in eight.

In our sample 46 per cent. presented a history of epilepsy, ten girls and sixteen boys, with 20 per cent. history in parents, 16 per cent. in second degree relatives, and the remaining 10 per cent. in more distant relatives. Five girls and four boys had a paternal history, four girls and eleven boys a maternal history, and one girl and one boy had a history on both sides of the family.

The average score-difference between the positive and negative cases is only 3 points, in favour of the former. When the positive cases were divided into degrees of relationship, test scores showed a consistent drop as the relationship of the epileptic relative became more remote.

The only positive associations worthy of note between family history of epilepsy and other variables are those with early neurosis and frequent seizures, the former being significant at the 2 per cent. level of confidence.

The average age of onset is one year earlier for the positive history cases but there is little difference between them in regard to duration of illness.

Age of Mother at the Birth of the Epileptic Child

The mean age of the mothers at the birth of the children in our survey was 30·6 years for both sexes of children. This seemed rather high. We were able to make a comparison with all Birmingham mothers, but only in respect of firstborn children (1953). The average ages were 29·5 and 23·5 years respectively, i.e. the mothers of the epileptic children were, on the average, six years older at the birth of their first child.

The mother's age, is not, however, significantly related to any of the other variables.

Abnormal Birth

Thirty per cent. of the children in the main sample were, apparently, born abnormally. In the absence of clear evidence from the doctor or midwife who attended the birth one cannot be absolutely certain in every case, but we were satisfied with the evidence on the whole.

Prematurity, instrumental delivery, breech birth, etc., are not always or necessarily regarded as abnormal these days but we have included such cases. Bridge (1949), one of the leading authorities on epilepsy in children, is strongly inclined to pay attention to any birth anomaly as a potential cause of cerebral damage, giving, as instances—distortion of skull with injury to intracranial vessels, uneven discharge of venous pressure, asphyxial necrosis of nerve-cells, venous haemorrhage, etc. He adds that forceps delivery and prolonged labour seemed to be the most important antecedents in cases of birth injury who subsequently developed epilepsy.

Our cases—ten girls and six boys—included three cases of instrumental birth, three of prolonged labour, and five premature births.*

The only test showing a significant score-deficit in the abnormal birth group is the psychomotor series, with -10 points.

There is a significant correlation ($P = \cdot 02$) between *normal* birth and dual seizures.

Two associations, both positive, approach significance, viz. with primogeniture ($P = \cdot 08$), and late milestones ($P = \cdot 09$).

Primogeniture

In our main sample fifteen cases—28·6 per cent.—were firstborn children, with equal division as to sex.

Their test scores show virtually no difference from those of the later born children.

The only notable positive associations with primogeniture are: abnormal birth, negative behaviour, and a positive history of epilepsy.

Milestones

Of fifty-three cases on which information was available thirteen or 25 per cent. were significantly late in their milestones, i.e. sitting up unassisted, walking, talking, etc. They were equally divided as to sex.

The test scores of these late developers differed insignificantly from those of the rest of the group, a rather surprising result.

Notable, but statistically insignificant associations between late milestones

* These were taken on a chronological basis. The tendency nowadays is to reckon prematurity in terms of birth weight, i.e. 5½ lb. or less.

and other variables were: early neurosis, abnormal birth, and negative behaviour.

Early Neurosis

Fifty-two per cent. of the main sample showed neurotic signs in infancy, with equal division as to sex.*

The neurotic traits recorded include enuresis, nail-biting, sleep-walking, temper outbursts, night terrors, screaming, stammering, biting clothes, feeding difficulties, head banging, etc.†

The average test scores of the neurotic and non-neurotic groups are almost identical.

The only two correlations worthy of note are with a positive history of epilepsy and late milestones. The average duration of epilepsy is one year longer for the neurotic cases.

Electrographic Data

EEG data were available for forty-one of our cases in the main sample. Three records were "Normal or Doubtful"; twenty-two were abnormal but not necessarily typical of epilepsy, while sixteen were grossly abnormal and strongly suggestive of epilepsy. We have made comparisons between the two broad degrees of abnormality.

Test scores show almost no difference between the two groups.

None of the other variables shows a significant association with grossly abnormal EEGs, but four show trends, viz. dual seizures, bad behaviour, attendance at residential school, and frequent seizures. There was an inverse relationship between grossly abnormal EEGs and six variables, notably positive history, grand mal seizures, and precipitants.

Scores on the reading, intelligence, and arithmetic tests showed only minor deficits in the grossly abnormal cases, while scores on the psychomotor series were actually higher in these cases.

Over-all "Saturation"

A final set of figures (Table VI), gives some indication of the degree of association between each of the above variables and all the others. Taking each variable in turn, and using the χ^2 tables, we have calculated the percentage incidence of each variable associated with it—and its opposite. These percentages were then summed and averaged. The positive values indicate high "saturations". It will be seen from the table that the brain-injured cases have the highest percentage of what may be called adverse associations, with dual seizures and bad behaviour next in order. Grossly abnormal EEGs, grand mal cases, and positive family history show the lowest percentages.

* Fuchs (1947) found signs of emotional conflict in all of 25 epileptic children. While he found no evidence for an "Epileptic Personality" he found that epileptic children are "generally nervous".

† Some of these traits could be undiscovered epileptic manifestations.

TABLE VI
Average "Saturation" of Certain Variables with Other Variables

Variable	Average "Saturation"		
	Positive Per cent.	Negative Per cent.	Balance
Cerebral injury	54.0	37.0	+17.0
Dual seizures	47.5	36.0	+11.5
Bad behaviour	44.8	33.7	+11.1
Late milestones	43.7	36.3	+7.4
Frequent seizures	42.0	35.9	+6.1
Negative behaviour	39.7	33.6	+6.1
Precipitants	37.1	31.9	+5.2
Petit mal seizures	40.0	37.3	+2.7
Early neurosis	37.0	35.2	+1.8
Abnormal birth	39.3	37.5	+1.8
Primogeniture	34.3	32.7	+1.6
Residential school	36.3	35.1	+1.2
Family history of epilepsy	34.2	37.1	-2.9
Grossly abnormal EEGs	35.4	39.4	-4.0
Grand mal seizures	31.2	43.2	-12.0

SUMMARY AND DISCUSSION

The fifty-six epileptic children in our main sample are below the intellectual standard of a matched control group of normal children, but not seriously so. Test score deficits range from 7.7 points in mental arithmetic to 12.2 points on verbal intelligence and psychomotor abilities. Compared with the distribution of epileptic children in Birmingham, where the survey was carried out, our sample is rather overweighted with residential school cases. If we adjusted our scores in the light of the Birmingham figures our average (composite) score would probably be about three points higher.

An analysis of the test results in more detail shows the epileptic children to be especially handicapped, in comparison with the control group, by slowness in thinking (which may be due partly to the effect of drugs, particularly the sedative drugs), immediate memory, and manual dexterity. Although they are backward, too, in numerical reasoning, they compare favourably in this respect with the control group.

The epileptic group is more heterogeneous than the control group, with about 60 per cent. more "scatter" in all tests.

Boys score higher than girls on all tests and in both the epileptic and control groups.

Dividing our main group according to type of seizure, we find that the grand mal cases score highest, with petit mal next. The cases with dual seizures, i.e. with grand mal and petit mal have the lowest scores. Their average quotients are 92.0, 83.6, and 82.7 respectively. Previous surveys have found petit mal cases to be the highest scorers.

The children with a positive family history score higher than those with a negative history, while the "Symptomatic" cases have lower scores than the rest of the group. The brain-injured children have the lowest scores of all, especially those cases with hemiplegia or quadriplegia as well as epilepsy. The average deficits of these last two groups are 15 and 30 points respectively.

Lower scores were found in the cases with an earlier onset and a longer duration of epilepsy, and also in the children with behaviour disorders. In regard to the latter it is not clear from our data which is cause and which effect, i.e.

the behaviour or the lower mentality. The relationship between them may be reciprocal.

The grand mal cases with frequent seizures score lower than the infrequent cases in all tests, with an average deficit of 12 points (18 points on arithmetic). On the other hand the frequent petit mal cases average 11 points higher than the infrequent cases.

It is interesting that the children with *grossly* abnormal EEGs show only a minor deficit in comparison with the children whose EEGs are abnormal but not necessarily suggestive of epilepsy. In the psychomotor series the former actually score higher.

The normal schools' epileptic children score 13 points higher than those at the residential school, a result which agrees with previous findings, and which, in our survey could have been expected, since the "better" cases are, in general, retained at normal schools.

It would appear that between 60 per cent. and 80 per cent. or more of epileptic children attend normal schools, according to the area in which they live. A greater proportion of epileptic children have gone to residential schools from London and Birmingham than from most other areas, especially rural areas. Nowadays the tendency is to retain as high a proportion of epileptic children as possible in the normal schools. The Ministry of Education's Regulations 1953 (1953) state that the children who require the special educational treatment provided in the residential schools are "those pupils who, by reason of epilepsy cannot be educated in ordinary schools without detriment to the interests of themselves or other pupils". In 1954 the Minister stated (1954) "No handicapped pupil should be sent to a special school who can be satisfactorily educated in an ordinary school. Where a special school is necessary, a day school is preferable if it offers a satisfactory and practicable solution."

The Sub-Committee on the "Medical Care of Epileptics" (1956) states (p. 19) "It should be the aim of all concerned to ensure that children suffering from epilepsy are, wherever possible, educated in ordinary schools", though they also suggest "the possibility of creating additional places at existing special schools and the desirability of establishing one or two national special schools for epileptic children with other handicaps".

In our residential group 71 per cent. are below the average range of intelligence (I.Q.s 91 to 110), as against 36 per cent. in the normal schools group. Sixty-four per cent. are below this range in reading and 82 per cent. in arithmetic—against 29 per cent. and 61 per cent. respectively in the normal schools. Donovan (1952) thinks that about 40 per cent. of the epileptic children in his (residential) school would still have to be educated in E.S.N. schools if they were not epileptic.

The advantages and disadvantages of a residential school for epileptic children were ably put by the late Dr. J. Tylor Fox (1947). But if we look only at the adverse factors found in our residential group, i.e. the duality, frequency, and severity of seizures, the grossly abnormal EEGs, malbehaviour, and brain injury, as well as the lower intelligence it is evident that the staffs of residential schools are coping with a difficult problem, and it is to their credit that one of them at least manages to discharge 40 per cent. each year with fits controlled and behaviour improved, and, at the same time, endeavour to attain normal educational standards. In some of our residential cases there was an increase in seizures and a deterioration in behaviour soon after their return home.

It is clear that test scores of epileptic children are related to many factors, some of which have been mentioned. There are others, such as loss of schooling,

the influence of drugs, anxiety, frustration, etc. If a child is unfortunate enough to have a combination of these factors, which may well happen since many of them are positively co-related, he could not be expected to compete on equal terms with others not so handicapped, and would have to receive special consideration.

Our survey shows that there is a higher incidence of behaviour disorders in epileptic than in normal children. The behaviour, like the cognitive functions, is also related to many other variables such as brain injury, sex, precipitating circumstances, grossly abnormal EEGs, type of seizure, frequent seizures, primogeniture, late milestones, longer duration of the illness, and others. These relationships indicate the complexity of the behaviour problem, and show how invidious it is to generalize about the "Epileptic Personality". Many such generalizations have been impressionistic. The present survey does not go far enough, and we must hope that others may be moved to carry out a more refined investigation on a larger sample.

As stated above, many of the epileptic variables are correlated (χ^2) with each other. Table V shows those associations which are at or near the level of statistical significance. Other notable associations have been dealt with in the appropriate sections. Table VI shows the average association of each variable with all the others, and here we find that the ones with the highest "adverse" associations are cerebral injury, dual seizures, and bad behaviour. At the other extreme are grand mal seizures, grossly abnormal EEGs, and positive history of epilepsy.

In conclusion we should draw attention to the difficulties encountered in trying to compare results of different surveys. There are many different interpretations placed on terms like "Idiopathic", "Cryptogenic", "Symptomatic", "Minor Epilepsy", and even "Epilepsy" itself. Different interpretations lead to different frequency classifications. The remedy for this confusion would be, of course, for workers to agree on a system of typology, such as that of Penfield and Jasper (1954), or some modification of it. Such a classification would entail a very thorough examination of cases, which would be beneficial from the viewpoint of treatment as well as research.

APPENDIX

Case-Histories of Brain-Injured Epileptics

1. P.K. (female), aged 9½, was the firstborn of identical twins, the other twin being normal. At the time of the birth the mother was 33 years old.

There is no history of epilepsy on either side of the family.

The birth was induced at eight months. The twins were said to have been locked and the birth, a breech one, was an instrumental one. The birth weight was three pounds. At birth the child had extensive injuries to the neck and left leg, which was stiff until the age of two.

Walking was delayed until age two. Bowel control was late.

The child had all the usual childish illnesses, including three attacks of pneumonia. During whooping cough there was a severe haemorrhage and the child had to be strapped upright for six weeks.

She was a timid child, with "inferiority feelings" and fear of the dark.

At age seven she was involved in a tramcar accident and although comparatively uninjured physically, suffered a severe emotional shock and "screamed continuously". About two weeks later seizures started. At one period the attacks—mainly akinetic—were said to be as many as fifty a day. She received many injuries as a result of these, including fractures.

At the time of testing she had been at the residential school for six months and had been free from seizures during that time. She was said to be about one year retarded and making little progress at school.

When tested she had an average quotient of 91, with a very wide range of individual scores—from 60 on arithmetic to 123 on the psychomotor series.

The EEG was "grossly abnormal".

2. J.W. (female), aged 16 years, was a firstborn and an only child.

There is no family history of epilepsy.

According to the mother, the birth was normal, but there was some evidence—e.g. facial paralysis—of cerebral damage or abnormality. The child suffered from strabismus and her skull was asymmetrical. She had little facial expression until she was about fifteen months old, and swallowed badly. She had loss of power in both hands ("weak wrists") and was obese.

All milestones were late.

She was described as a very nervous child, afraid of noise, flames, etc. She sleep walked and had various tics. She also had a speech defect, mainly concerned with the enunciation of consonants.

At the age of four she saw her grandfather fall down and die and seizures started soon afterwards. She suffered from dual attacks at the time of the survey.

At five years old she was under observation in hospital, and was found to have a "nuclear agenesis of the sixth and seventh nerves and the fifth right cranial nerve".

The child's education started in a normal school where she graduated from the C to the B-stream in her fourth year. Her behaviour was apparently satisfactory though she had feelings of inferiority. At the age of fifteen she was transferred to the residential special school.

Her average test quotient was 59, with a range from 38 on the psychomotor series (the lowest recorded in the survey) to 86 on reading.

The EEG was normal.

3. H.P. (male), aged 14½, was the third child, the mother being aged 29 at his birth. The family history was negative as to epilepsy.

Birth was premature at seven months. His twin, born after him and stillborn, was said to have been lying on him *in utero*. There was some cerebral injury at birth and the boy has a hemiplegia of the left arm and leg.

All milestones were late.

As an infant he was "highly strung", bit his clothes, and sleep-walked.

The onset of epilepsy was at one year, and was attributed to the first air raid warning! His attacks were usually severe. Just before they came on he was violent and uncontrollable but afterwards was quiet and docile. He was easily excited.

He attended normal school until the age of twelve when he was excluded from school because of his attacks and troublesome behaviour. On one occasion he attacked his father with a knife.

He was transferred to the residential school and his behaviour gradually improved and his seizures diminished. In the six months prior to testing he had been free from attacks and was returned to a normal school, where his report said that he was still free from fits but his reactions were slow. He was moody and, at times truculent, though he tried to be pleasant. His position in class was twenty-sixth out of twenty-seven.

His average quotient on tests was 84. His worst performance was on the psychomotor series with a quotient of 70. He was somewhat handicapped in this because of the hemiplegia. His best performance—95 points—was on verbal intelligence.

No information was available about the EEG.

4. A.W. (male), aged seven years, was a firstborn child, his mother being 23 at the time of the birth.

The father gave a history of seizures in infancy.

Birth was apparently normal and milestones too, but the boy was said to have been overactive and to have bitten his nails a lot.

At the age of one year he fell and damaged the back of his head, and "seemed to stare" afterwards. He was treated in hospital at the neurological unit. Six weeks later he had an epileptic attack. Following this he was free from fits until the age of four when he had another attack during measles. At the age of 4½ he had a seizure in the road and was knocked down by a bicycle, sustaining damage to the base of the skull, which again had to receive attention in hospital. There was an operation on the right temporal lobe.

The child attended normal school at first, where he was said to be aggressive, demanding and uncontrollable; playing up and dashing around wildly. There were "endless rows" between the parents about the boy, and he was eventually sent to the residential school.

The report there stated that he was a very difficult problem, being sadistic, with violent tempers and prone to using foul language. Later he showed some improvement in both behaviour and in class work.

He had had no seizures for more than six months.

His average test score was 67, with individual scores ranging from 50 on reading to 80 on verbal intelligence.

The EEG report was "normal or doubtful".

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NIGHT VISION AND PSYCHIATRIC DISORDERS: A REVIEW OF EXPERIMENTAL STUDIES

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I. INTRODUCTION

IN recent years a number of studies have been undertaken in which the "night vision" or dark-adaptation of psychiatric patients has been compared with that of normal subjects. These studies have their origin in a wartime observation that the incidence of "night-blindness" among neurotics was higher than among normal Service personnel. Evidence of functional disorders of vision is of interest from several points of view, psychiatric and ophthalmological as well as psychological and physiological. The aim of this article is to make a critical evaluation of the results so far obtained, determine what generalizations are possible and consider implications for further research.

The paper is divided into four parts. The first involves a description of experiments and results, the second deals with a psychophysical evaluation of the results, the third considers hypotheses as to possible mechanisms involved from a psychophysiological point of view, and the fourth implications for further research.

II. EXPERIMENTAL STUDIES

The first study to be considered was undertaken by Livingston and Bolton (67) using the Livingston Rotating Hexagon test. This consists of an apparatus for presenting letters and objects (aircraft, arrows, ships, etc.) for identification under conditions of low illumination. In all, four tests are given at different levels of illumination, ranging from 0.0012 to 0.00015 equivalent foot-candles, each test involving six letters and two objects. Subjects are dark-adapted for forty minutes prior to the test and record their answers on a Braille card in the dark, one minute being allowed for this purpose for each test. From the distribution of scores for 50 R.A.F. personnel and 50 neurotic patients, the authors conclude that the neurotic group as a whole produce a far greater number of poor results. This conclusion seems justified from the data shown in Figure 1. Within the neurotic group anxiety states (of whom there were 30) appeared to give particularly poor results as compared with depressives.

Rees (93) compared the results of 36 anxiety states, 33 (neurotic) depressives and 27 hysterics with those of 6,062 R.A.F. aircrew personnel and found the same general picture for the total neurotic group as Livingston and Bolton (67) obtained, as will be seen from Figure 2. In line also with a tendency noted in Livingston and Bolton's study is the fact that anxiety states do worse than other neurotic patients. Thus, when patients are graded according to a list of categories drawn up by Steadman (113), it is found that only about 3 per cent. of anxiety states have "above average" grades, compared with 15 per cent. of depressives and 18 per cent. of hysterics. At the same time, 81 per cent. of anxiety states have "below average" scores, compared with 67 per cent. of depressives and 63 per cent. of hysterics.

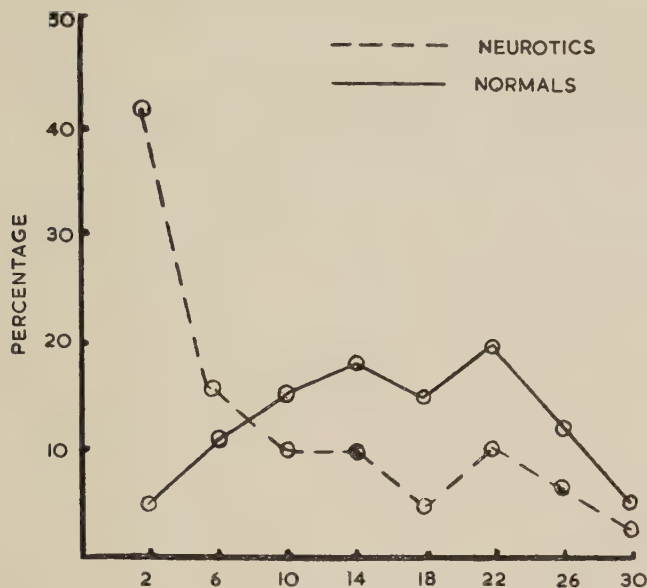


FIG. 1.—Frequency distribution of scores. (Livingston and Bolton (67).)

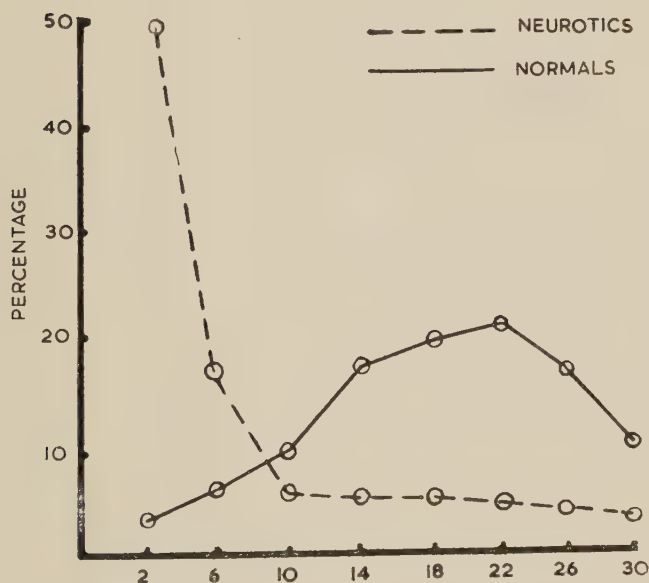


FIG. 2.—Frequency distribution of scores. (Rees (93).)

Age and intelligence seem to be relatively unimportant factors in determining these results, the correlation with intelligence being almost zero and with age about -0.4 . Unfortunately, both correlations were calculated for the neurotic group only; information is not available on the normal group nor are the age and intelligence of the normal group given. However, even though we cannot make a precise estimate of the effect of either variable, such evidence as

is available gives no reason to suppose that intelligence and age could account for differences of the size observed.

Rees comments on the fact that the comparison is based on neurotic soldiers and R.A.F. aircrew personnel. The latter do not constitute an "ideal" control group, for standards of enlistment in the R.A.F. are more rigorous than in the army so that the "R.A.F. results may be higher than those of the general population of the British Isles". As data on the general population are not available Rees presents results for the men of an A.A. unit who had taken the test. This army group obtained lower scores than the R.A.F. group, but there is still a difference between them and the neurotic group. In the absence of data one can only speculate as to the reason for this difference; it may be due to age, intelligence, "neuroticism", or other factors.

An unfortunate feature both of Livingston and Bolton's (67) and Rees's (93) study is the fact that no information is provided concerning performances of subjects at each of the four different luminance levels. Although not necessarily a serious omission from the psychometric viewpoint it makes psychophysical evaluation of their investigations difficult, for the range of luminance covered by the test involves both photopic and scotopic vision; to what extent the test scores are determined by the former or the latter cannot be determined.

Himmelweit, Desai and Petrie (50) compared the "dark vision" of 105 neurotic patients (all returned P.O.W.s) with that of 93 surgical cases, matched in respect of age and educational background. The apparatus used for testing dark vision was the U.S. Radium Plaque Adaptometer. After being dark-adapted for forty minutes the subject was required to give the direction, up, down, left or right, of the letter "T" appearing against a uniformly illuminated background. Details concerning the luminance of this background are not given, but it would appear from data which the author has obtained from the Medical Research Laboratory reports of the U.S. Navy that the luminance level was probably about $10^{3.9}$ micromicrolamberts, i.e. toward the final brightness threshold of "rod" vision. A red fixation point was used so that the test-object fell outside the fovea but the precise retinal location is not specified. The visual angle subtended by the test-letter appears to have been large (about 3°)* judging from M.R.L. reports, but the authors do not specify the size. The procedure was to change the position of the test-letter twenty times during the test and to allot one mark for each position correctly reported.

Details of mean scores and scatter for the two groups are not provided but there appears to have been a very slight tendency for the neurotic group to have lower scores for a small positive correlation ($.27$) was found between the dark vision test and the "normal/neurotic" dichotomy. The authors note that their test does not discriminate so well as did that of Rees. This they explain as probably due to the fact that the difference in "neuroticism" between their two groups was smaller than in Rees's study. The discrepancy could, however, also be due at least in part to differences in experimental conditions (luminance level, size and type of test object, viewing conditions, etc.). These differences are completely ignored by the authors who appear to assume that they were using essentially the same test of "dark vision" that Rees used.

Gravely (45) made use of yet another different set-up to study differences between 62 neurotic soldiers and 94 normal controls (R.A. personnel from a re-allocation centre). The adaptometer she used covered a luminance range from 3.961×10^{-7} to 7.847×10^{-3} e.f.c., i.e. the entire range of luminance examined

* Judging from the acuity value ($.025$) given in M.R.L. reports the test-letter would make minimal demands upon detail perception.

in the usual studies of the dark-adaptation curve. Gravely however made use of only the upper part of this range.*

Subjects were required to report upon the position of a black triangular shaped test object which could be rotated into any one of eight different positions against a uniformly illuminated field subtending about 4° visual angle, the viewing distance being about six feet. No fixation point was used but the subject was encouraged to employ peripheral vision when perception became difficult. Starting at a luminance of $10 \cdot 37 \times 10^{-4}$ e.f.c., intensity thresholds were recorded for the perception of the test-object at 3, 6 and 10 minutes after the room light was switched off. In accordance with the usual practice (i.e. in the British Navy where it is used) in administering this test, no period of controlled light-adaptation preceded the dark-adaptation measurements, and thresholds were recorded in terms of brightness grades.

Significant differences were found between the mean dark vision grades of normal and neurotic groups, the extent of the differences increasing with time in the dark from about 1.5 grades at 5 to nearly two grades at 10 minutes. Variances for the two groups were about the same at three minutes but thereafter the neurotic group had a larger variance than the normal.

Very small differences were found also between the mean grades of 31 anxiety states and 31 hysterics comprising the neurotic group, the former having lower thresholds, but the differences did not reach an acceptable level of statistical significance. The variance of the anxiety states was about twice as large at 5 and about three times as large at ten minutes as that of the hysterics. Neither age nor intelligence appeared to influence the grades of the neurotic group, as shown by near-zero correlation coefficients.

Granger (38) compared intensity thresholds for the same type of test-object as Gravely had used of 106 normal subjects (soldiers from the same re-allocation centre as Gravely's came from) with those of 20 neurotic and 20 psychotic patients from the Bethlem Royal and Maudsley hospitals. Threshold measures were taken at five-minute intervals over a thirty minute period of dark-adaptation, the criterion of success at a given brightness level being two consecutive correct reports of the position of the test-object. There was a slight tendency for neurotics to have lower thresholds than psychotics but the differences failed to attain statistical significance. Differences between normals and psychotics were highly significant at 5, 10, 25 and 30 minutes ($P < 0 \cdot 005$, $< 0 \cdot 02$, $< 0 \cdot 05$, and $< 0 \cdot 02$ respectively) but only toward the latter part of the adaptation period did differences between normals and neurotics reach statistical significance ($P < 0 \cdot 05$ and $< 0 \cdot 02$ at 25 and 30 minutes).

Distributions of individual thresholds at each level roughly follow a normal distribution for both normal and psychiatric groups with variances reducing in size as adaptation proceeds.

The study has not been repeated with psychotics but a second group of 20 neurotics taken from a different mental hospital gave essentially the same mean thresholds and variances as the first group over the whole of the adaptation curve.

Worth noting is the fact that the mean final threshold of the normal group reached after thirty minutes was $0 \cdot 25$ log unit $\mu\mu\text{L}$ higher than that of a group of naval ratings.† Testing conditions were different in that the naval group did

* Calibration data are not available for the particular adaptometer which Gravely used but from data that are available for a similar type of instrument it appears that she was probably working between luminance levels of about $6 \cdot 0$ – $4 \cdot 0$ log $\mu\mu\text{L}$.

† Data were kindly supplied by the Admiralty Research Laboratory.

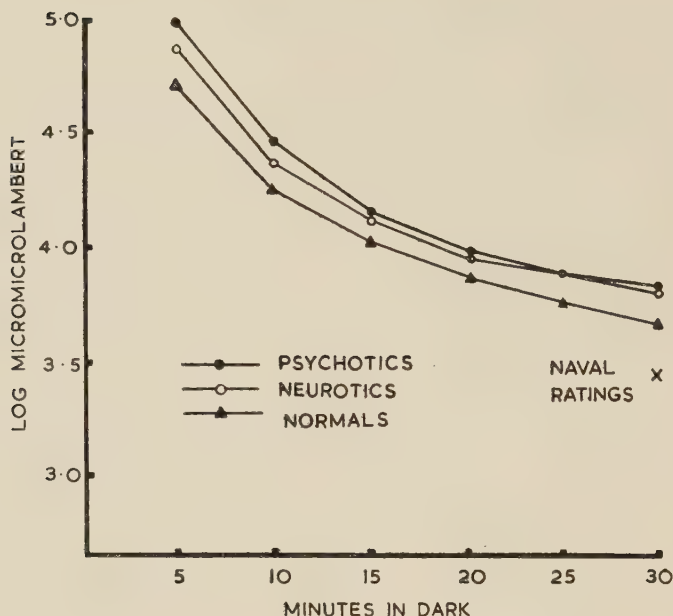


FIG. 3.—Dark adaptation curves. (Granger (38).)

not have thresholds taken throughout the thirty-minute period but only at the end. Although repeated threshold determination throughout the test would tend to raise the thresholds of the army group due to the effects of light-adaptation it is very improbable that this factor alone would account for the difference between them and the ratings. If the final threshold of the psychiatric groups is compared with that of the naval group the difference amounts to as much as 0.4 log unit (i.e. the patients need about two and a half times as much light to see the test object).

As there were significant differences in age between the groups, correlations were determined between age and intensity thresholds at 5 and 10 minutes after the beginning of dark-adaptation.* The correlation in the former case was -0.033 and in the latter -0.216 , indicating that age had no significant effect. These results accord fairly well with previous findings (96, 114) in which only small positive correlations have been found with age although it should be noted that McFarland and Fisher (76) using the Hecht-Shlaer adaptometer have recently reported a very high correlation ($.89$) between age and the final (rod) threshold.

Correlations with intelligence, as measured by the Nufferno test, were 0.143 and $+0.353$ for intensity thresholds at 5 and 30 minutes respectively. No significant differences in intelligence existed between normals and psychiatric patients, so intelligence cannot explain the results. Correlations with visual acuity as determined by the Snellen chart were small ($.033$ at 5 minutes and $.237$ at 30 minutes) in line with expectation. Also in line with other findings (48, 49) was the relatively small correlation of 0.44 between intensity thresholds at 5 and at 30 minutes. An incidental finding of this study, which formed part of a much larger research project, was the very low correlation between

* Unfortunately no correlations with age are available for the final thresholds at 30 minutes.

thresholds at 5 and 10 minutes and other sensory and perceptual tests of a visual nature (29).

Apart from this study the only other experiment in which psychotic patients have been used appears to be an unpublished study by Brown and Marshall (15); using a Crookes Adaptometer they found no evidence of any gross disturbance in the dark-adaptation of six schizophrenics but unfortunately they had reason to believe that their instrument was not consistently giving the specified amount of light. In a later experiment Marshall and Day (81) investigated the ability of 12 schizophrenic patients to discriminate bar grids, with detail ranging from 10' to 40', presented against a test-patch subtending 10° visual angle. Viewing was binocular with normal pupils and "little or no control over fixation". Compared with a group of 13 normal subjects all 12 schizophrenics had higher intensity thresholds for perception of the bar grids. Both patients and normals had approximately 6/6 vision. While the investigation is not yet complete intelligence as measured by the Binet or Wechsler seems, according to the authors, unlikely to account for differences between the two groups.

Granger (39) in a further experiment on dark-adaptation compared the dark-adaptation curves of 20 normal and 20 neurotic patients using the Crookes Adaptometer and again found evidence of differences between the two groups, the patients having higher thresholds throughout the "rod" portion of the adaptation curve, for recognition of the position of a 7° test-object in the form of an arrowhead. In accordance with the standard procedure for administering the test, viewing was not controlled by means of a fixation point. Before the test the eyes were adapted to a luminance of 750 ml. at an approximate colour temperature of 2,500° K for a period of five minutes. Results are shown in Figure 4, the extent of the difference between groups ranging from about 0.2 to 0.5 log unit at different levels of adaptation. Differences reached statistical significance at the .05 level only between 8 and 22 minutes.

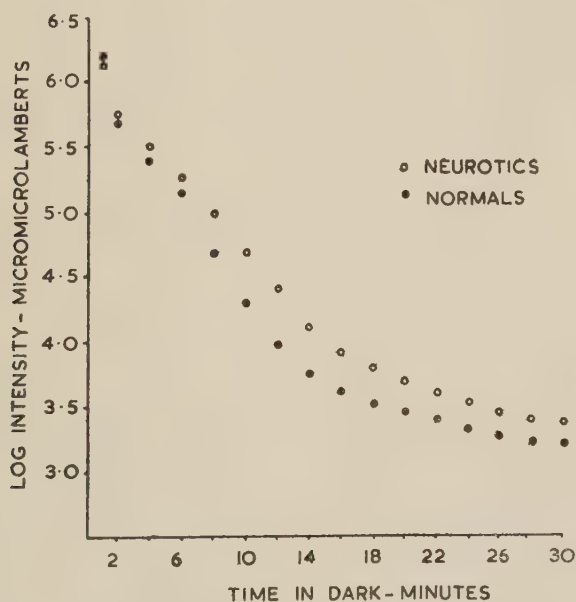


FIG. 4.—Dark adaptation curves. (Granger (39)

In addition to differences in means other interesting trends appeared, anxiety states tending to show "plateaux", periods during which sensitivity did not improve, hysterics tended to have high final rod thresholds and psychopaths low final thresholds. Unfortunately, the composition of the neurotic group did not enable any definite conclusion to be drawn about these various sub-categories.

In a later experiment Granger (41) compared 10 anxiety states with 10 hysterics, thresholds being obtained for perception of a 3° test-field displaced 7° degrees horizontally from a fixation point in the nasal field over a thirty-minute period. The adapting luminance prior to the dark-adaptation test was 270 ml. viewed for five minutes.

Comparison of the mean dark-adaptation curves for the two groups showed a significant difference in level, the curve for the hysterics being displaced upward along the intensity axis by amounts varying between 0.2 and 0.35 log unit (see Figure 5). A significant tendency was also found for the hysterics to take longer to recover from the effects of light-adaptation in the early stages of dark-adaptation. While the mean adaptation curve of the hysterics was normal in shape, the individual curves of several hysterics tended to have a shallower slope.

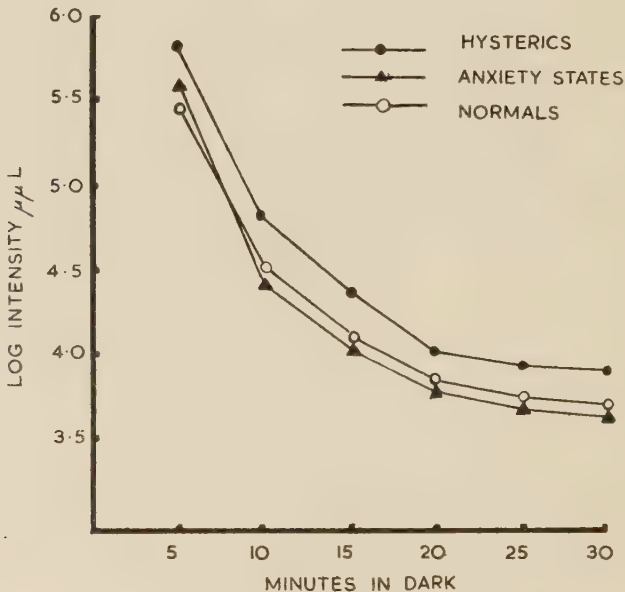


FIG. 5.—Dark adaptation curves. (Granger (41).)

Data available from ten normal subjects who had also taken the same test when corrected for the effects of age (they were a younger group) suggested that the curve for a group of normal subjects of the same age as the neurotic groups would fall about midway between the curves of the anxiety states and hysterics.

Two further studies which have in common the fact that thresholds were taken only at the very beginning of the dark-adaptation period were undertaken by Janda (56) and Granger (40). In Janda's study the general object was to observe the effects of the subject's "general or chronic level of stress" upon night vision by comparing "high stress" (neurotic) with "low stress" (normal) subjects. Two experiments were carried out; in the first of which 25 neurotics

and 25 normals took part, both groups being given the MMPI as part of the selection procedure. Besides being classified on the basis of high and low stress, subjects were further sub-divided according to their usual method of handling stress into "expressors" whose mode of handling stress is characterized by frequent and abundant expressions of manifest anxiety and "repressors" who attempt by various means to prevent the direct expression of anxiety.

Following light-adaptation for three minutes to a luminance of 1,600 foot lamberts the subject viewed a rectangular-shaped test object 2 in. $\times$ $\frac{5}{8}$ in. in size at 0.008 foot L., the viewing distance being 9 in. Some control of fixation was provided by asking the subject to look in the direction of his finger so placed on a small knob at the side of the test-light that the test-object fell at about 14 degrees from the fovea. The time in seconds was recorded from cessation of light-adaptation to correct report of the test-object's position (horizontal, vertical or diagonal). A second trial was given about 15 minutes later.

In the second study instructions were changed to allow "flexible fixation" and meaningful forms as well as the simple rectangular test-object were used. These forms, depicting various military objects, were presented immediately after correct report of the simple test-object's position. Two groups of subjects, normal and neurotic, took part in the experiment, each consisting of 14 "expressors" and 14 "repressors", selection again being based on psychiatric diagnosis and the MMPI.

Results from the first study showed a slightly longer perception time for neurotics as compared with normals, although the difference was not significant statistically. The same tendency was found on re-test. Mean perception time for expressors was longer than for repressors on the first trial, but shorter on the second. There was a significant tendency for all subjects to have shorter perception times on the second trial, but the relative improvement was larger for expressors than for repressors.

In the second study the mean perception time for stimulus position of the neurotic group was again longer than that of the normal group, but the difference in means was significant only on the first trial. As in the first study, expressors as a group had longer perception times for stimulus position than did the repressors on the initial trial, but shorter perception times on the second; in neither case were differences statistically significant. Particularly long were response times of neurotic expressors on the first trial.

No difference was found between mean perception times of normals and neurotics on the first trial for form identification, but there was a (statistically insignificant) tendency for neurotics on re-test to have longer perception times than normals. Results for expressors and repressors tended to parallel those obtained for stimulus position, perception times being longer for expressors on the initial trial and shorter on re-test; again, differences between the two groups were insignificant. As in the first experiment, perception times were in general shorter for all groups on re-test and relative improvement was greater for expressors than repressors.

Janda makes comparisons between his results and those of Rees (93) and considers they confirm Rees's major finding that the night vision performance of normal subjects is superior to that of neurotics, but points out that this finding applies only to the first trial and using uncontrolled viewing. Under these conditions it is the high stress expressors, whom Janda considers similar to Rees's dysthymics (anxious and depressed patients), who are most impaired.

Rees's finding, that hysterics were not as impaired in "night vision" as dysthymics is, in Janda's view, paralleled in his own study, in that neurotic repressors had significantly lower stimulus position thresholds on the first trial than did neurotic expressors, under uncontrolled viewing conditions. His failure to confirm Rees's findings on form identity thresholds Janda considers may be due to differences in experimental conditions. It is certainly true that there were marked differences in conditions; in fact the differences are so great that it is very doubtful if the results of the two studies can be compared except in terms of a very broad and rather vague concept such as "night vision".

Similar in certain respects to Janda's study was an investigation by Granger (40) in which in response to a request for a short psychiatric screening test a preliminary technique was tried out on 100 neurotic and 40 normal subjects. Following three minutes' light-adaptation to an adapting-field of 675 ml. at a colour temperature of 6,500° K, the subject reported upon the position of a faintly-illuminated triangular-shaped test-object of 4° angular subtense. The luminance of the test-field was set at $10^{-3.04}$ mL and was viewed binocularly at a distance of six feet. The time in seconds between the cessation of light-adaptation and correct verbal report on the position of the test-object (up, down, right or left) constituted the subject's score.

Marked differences were found between normal and neurotic groups, the latter having longer perception times. Differences were particularly striking at the upper and lower ends of the time scale; whereas 42 per cent. of normals had perception times of less than 100 seconds, only 6 per cent. of neurotics fell into this category and whereas 34 per cent. of neurotics had perception times of 300 seconds or more no normal had a perception time exceeding 300 seconds. For convenience in making a statistical comparison a log transformation was used and a t-test of the difference in mean log times gave $P < .001$ (see Figure 6).

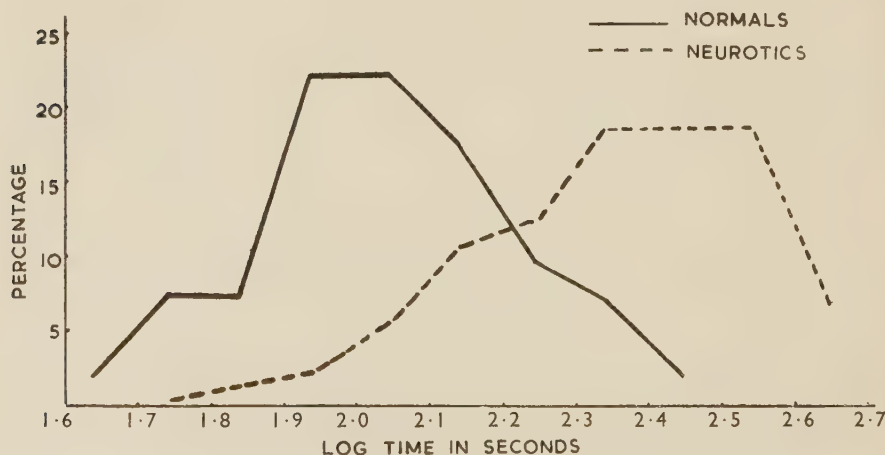


FIG. 6.—Frequency distribution of perception times. (Granger (40).)

An almost zero correlation was found between "perception time" and intelligence, as measured by Progressive Matrices, for the neurotic group but a small positive correlation ($.21$) was found with age. There was no significant difference in age between normal and neurotic groups.

III. PSYCHOPHYSICAL EVALUATION OF RESULTS

Perhaps the most striking feature of the researches reviewed above is the diversity of techniques and experimental conditions that have been used. Certainly all the studies involve "tests" of "night vision" in that they deal with visual functioning under conditions of low illumination, but in spite of this general similarity there are marked differences between them in respect of size and type of test-object, state of adaptation of the subject's eyes, criteria of "visibility", method of threshold measurement, range of luminance, etc.

This variation in conditions is particularly apparent in the case of luminance. By converting the luminance levels used in the different experiments to a common unit (log micromicrolambert) and plotting these ranges it is clear for instance, that Himmelweit, Desai and Petrie's test of "dark vision" is, psychophysically, a very different test, from that of Rees. Whereas Rees was working at luminance levels between $10^{6.1} \mu\mu\text{L}$ and $10^{5.2} \mu\mu\text{L}$, involving both "photopic" and "scotopic" vision, Himmelweit, Desai and Petrie's test was administered at a much lower luminance level of about $10^{3.9} \mu\mu\text{L}$. Difference in luminance level is not the only difference in experimental conditions between the two experiments; there were other differences in respect of the size, shape and type of test-object, retinal area tested, viewing distance, method of recording responses and so on.

Numerous other differences occur between experiments. Thus Janda's study of form discrimination was carried out in the early stage of dark-adaptation at a luminance level of approximately $10^{6.9} \mu\mu\text{L}$ immediately following a period of controlled light-adaptation whereas Rees presented forms for discrimination to fully-dark-adapted subjects. Gravely made threshold determinations for perception of the position of a test-object during a ten-minute period of dark-adaptation, without controlling previous light-adaptation or fixation and using binocular vision, whereas Granger (41) employed a fixation point to test a specific retinal region, used monocular vision, and his subjects were merely required to report upon the presence of a circular patch of light during a thirty-minute period in the dark, following a period of controlled light-adaptation.

The great diversity of techniques makes comparison between one study and another difficult, just as comparison was made difficult between the numerous wartime studies devoted to the search for a "test of night vision", as Verplanck (8) has pointed out. Marked differences in experimental conditions have been ignored by some investigators when making comparisons between their own investigation and someone else's. Thus, Himmelweit, Desai and Petrie, in comparing their results with those of Rees, seem to assume that they are using essentially the same "test" of "dark vision" as was used by Rees, although as we have seen above there were many differences between the techniques employed in the two investigations.

A second feature of much of the research is its purely psychometric nature, the chief object of many studies being to discriminate between normal and psychiatric groups by means of a "test" of "dark vision". This is, of course, a perfectly legitimate aim, but in certain cases it has tended to make psychophysical evaluation difficult. This is seen for instance in Rees's study where the total score for the test is obtained by summing scores of performances at four different luminance levels. The luminance range covers both "photopic" and "scotopic" vision, but it is impossible to determine whether differentiation occurred primarily under photopic or scotopic conditions. Particularly difficult

to evaluate from the psychophysical viewpoint is the study by Himmelweit, Desai and Petrie (50) owing to the almost complete lack of information concerning experimental conditions. Further difficulties arise in connection with Gravelly (45) and Granger's (38) experiments in which scores were allotted in terms of "brightness grades" rather than in terms of units of luminance, although it is possible to make an approximate estimate of the equivalent values in terms of $\log \mu\mu\text{L}$.

In the third place, judged in relation to the standards of research customary in visual psychophysics, all of the investigations leave much to be desired in the way of physical, physiological and psychological controls. To take but one variable, the light-history of the eye immediately prior to the test, in several studies this factor is completely uncontrolled yet it is known to affect the subsequent course of dark-adaptation, particularly in its early stages. Or, to cite another example, the lack of control over viewing conditions in many experiments means that some subjects may have employed more sensitive areas of their retinae than others when viewing the test-field.

Fourthly, there is an almost complete absence of re-test data and repeat studies, except in the case of Janda's (56) and Granger's (39) experiments. There are practical difficulties involved in re-testing the same patient on different occasions in that even a single session of the test may be quite stressful but the lack of re-test data nevertheless makes evaluation of results difficult. It is known that there may be considerable day-to-day variation in individual threshold measurements of as much as 0.3 log unit (49) and reliability coefficients of "night vision" tests seldom exceeded 0.8 in the numerous wartime studies that were carried out, usually they ranged between 0.6 and 0.8 (8).

Fifthly, considerable heterogeneity has been observed in the reactions of psychiatric groups under some experimental conditions. Thus, in Granger's (39) study differences were found within a group of neurotics with regard to the shape of their dark-adaptation curves which limit the descriptive value of comparisons based on level alone. Lack of homogeneity in the experimental data often makes averaging of results in visual experiments of dubious value, as Crozier (65) has pointed out.

Finally, there is a complete lack of objective data on the more general psychological characteristics of the groups to which the night vision measurements can be related. Groups are classified only on the basis of crude descriptive categories such as "neurotic" and "psychotic". How great a limitation is the use of psychiatric labels depends on one's viewpoint. Regarded as an anomaly, in the sense of an apparent deviation from the type of dark-adaptation curve ordinarily obtained under a particular set of experimental conditions, the dark-adaptation function of a patient labelled "hysteric" becomes a starting point for an enquiry into the mechanisms responsible for the "discrepancy". Whether these mechanisms have any significant relation to psychiatric criteria and labels or implications for psychiatry is another matter. This would be the viewpoint of visual research. From the viewpoint of personality research the position is perhaps less satisfactory owing to the absence of any measurements on personality variables although the studies may still be regarded as suggestive for further research (see below in section dealing with Sensory Factors).

Bearing in mind the above comments and criticisms, the next problem is to consider the experimental conditions under which differences between normal and psychiatric groups occur and to what extent results of the various studies are consistent with one another. Considering first differences between neurotics and normals, Gravelly (45) found evidence of higher intensity thresholds in the

neurotic group over the first ten minutes or so of dark-adaptation, at luminances below about $5.5 \log \text{ unit } (\mu\mu\text{L})$. Her results are consistent with those of Granger (38) who found evidence of higher thresholds also over the subsequent twenty-minute period of dark-adaptation. It is difficult to be precise about the extent of the difference in the case of Gravely's experiment, but it would appear to be of the order of approximately $0.15\text{--}0.20 \log \text{ unit } (\mu\mu\text{L})$. Threshold differences in Granger's study (repeated with a second group of neurotics) varied between 0.25 and $0.40 \log \text{ unit}$. Consistent also with Gravely's (45) and Granger's (38) results are those of Granger (39) using a somewhat different experimental procedure. In the latter experiment the dark-adaptation curve for the neurotic group was shifted along the log-luminance axis by amounts varying from 0.2 to $0.5 \log \text{ unit}$. The results of Himmelweit, Desai and Petrie's (50) experiment, although difficult to evaluate in view of the lack of information concerning experimental conditions, also seem to be in line with the other findings in that some tendency was found for neurotics to have higher intensity thresholds at a luminance level of (probably) $3.9 \log \text{ unit } (\mu\mu\text{L})$, i.e. toward the final "rod" threshold of the adaptation curve.

Following this statement about differences between neurotics as a group, and normals, it is necessary to consider the results of Granger's (41) experiment in which it was found that although hysterics had higher thresholds than normal subjects (on an average of about $0.2 \log \text{ unit}$), anxiety states had normal or superior sensitivity and adaptation curves of normal shape. These results were obtained using a different threshold criterion from the type used in the studies referred to in the previous paragraph, viz. a simple light stimulus rather than a geometrical test-object, so it seems that differences may occur *within* the neurotic group depending upon experimental conditions for threshold determination. Anxiety states do not apparently have higher thresholds for light *per se*, in fact they tend to have lower thresholds, although they *may* have higher thresholds than normals when the visual task makes demands upon form perception in the sense of requiring the location of a simple geometrical object against a dimly-illuminated background. In view of the fact that the intensity thresholds for form location do not coincide with those for awareness of light as such but tend to be higher (55, 82), the results of Granger's (41) experiment with light thresholds are not inconsistent with the experimental results obtained using form location as threshold criterion (Gravely, 45).

Nor are they inconsistent with the results of experiments by Livingston and Bolton (67) and Rees (93) on form discrimination at low intensities in which they found a tendency for neurotics to have inferior performances to normals. It is perfectly possible for a person with a low absolute threshold for light to have a high threshold for form discrimination and identification (20, 55). The poor performance of anxiety states on the Livingston Rotating Hexagon test are therefore not inconsistent with their normal (or superior) performances in Granger's (41) experiment. The inferior performances of hysterics on the Livingston test are also not inconsistent with their having higher light thresholds in Granger's study for perception of the test-objects must depend in the first instance upon light sensitivity.* An individual with a high light threshold would tend to see the illuminated panel bearing the test-objects less readily than an individual with a normal light threshold. On this suggested analysis, two individuals could have the same (low) score on the Livingston test for different

* Although there is no direct evidence for *superior* form discrimination in hysterics, it may be significant that Gravely (45) found a higher C.F.F. in hysterics than in anxiety states, using a test batch of photopic intensity.

reasons, in the one case due to impairment of the light threshold and in the other due to impairment of form perception. It would seem then, to return to the original statement about higher intensity thresholds in the neurotic group, that differentiation between neurotics and normals depends upon the composition of the neurotic group (i.e. the relative numbers of anxiety states and hysterics) and the type of threshold criterion employed.

So far consideration has been given to results obtained mainly at "scotopic" levels of illumination (i.e. conventionally below about $10^5 \mu\mu\text{L}$). Data are also available for the higher levels at which "cone" vision is functioning or at the cone-rod transition point where a change-over from "photopic" to "scotopic" mechanisms occurs. Granger (40) found that following a period of light-adaptation, neurotics took longer than normals on the average, to perceive a test-object presented at a pre-determined luminance. His results tend to be broadly consistent with those of Janda (56), although whereas Granger obtained highly significant differentiation between groups, Janda obtained very few significant differences. Among the differences in experimental conditions between the two experiments, light-adaptation and test-object luminances may be important factors in accounting for the less significant results of Janda. Whereas Granger used a pre-adapting field of 675 ml. and a test-field luminance of $10^{6.04} \mu\mu\text{L}$, Janda employed a pre-adapting intensity of 1,722 ml. and a test-object of $10^{6.9} \mu\mu\text{L}$. The effect of the higher intensities used in Janda's experiment would mean that his threshold measurements relate to an "earlier" part of the cone adaptation curve than Granger's. That the relative values of the pre-adaptation and test-field luminances may play a part in determining differences between normals and neurotics is indicated by the fact that in Granger's study (39) no differentiation occurred between normals and neurotics until about $10^{6.2} \mu\mu\text{L}$ and not until the test-field luminance was below $10^{6.0} \mu\mu\text{L}$ did the differentiation become at all significant.*

In a further study (41) involving the detection of light rather than location of a test-object it was found that hysterics had longer perception times than normals, following a period of light-adaptation, whereas anxiety states had normal perception times. As in the case of the results obtained at lower levels of illumination, it seems as if the type of threshold criterion may be an important factor in producing differences *within* the neurotic group.

Information on psychotic patients is meagre, but it appears from Granger's (38) study that psychotics also tend to have higher intensity thresholds than normals and from Marshall and Day's (81) study that schizophrenics have higher thresholds for perception of visual detail during dark adaptation.

Summarizing the discussion, the following general conclusions emerge, *provided trends in the data are taken into account as well as statistically significant findings*:

1. Psychiatric patients tend to have higher intensity thresholds than normals during the course of dark-adaptation. Their dark-adaptation curves, although of the same shape as the normal, tend to be displaced upward along the intensity axis by amounts varying between 0.2 and 0.5 log unit. In other words, patients require between one and a half and three times as much light for perception as compared with normal subjects.

2. Evidence for differences between normal subjects and patients is clearest under conditions of scotopic rather than photopic vision (i.e. at luminance levels below about $10^6 \mu\mu\text{L}$).

* This finding is not apparent from the data plotted in Figure 4 where the two groups were compared in terms of intensity level.

3. The time taken to recover from the effects of light-adaptation as determined by their initial threshold responses tends to be longer in psychiatric patients than in normal subjects.

4. Differences *within* psychiatric groups occur depending upon the threshold criterion. Anxiety states have normal or superior light thresholds, but impaired thresholds for form perception, whereas hysterics have high light thresholds and results are consistent with the hypothesis that their form thresholds are normal or lower than normal.

IV. POSSIBLE PSYCHOPHYSIOLOGICAL MECHANISMS

Having attempted a psychophysical analysis of the various test results and found a certain amount of consistency in the experimental data, the next problem is to consider some of the possible psychophysiological mechanisms that may underlie the observed differences. First* to be considered is the possibility that sensory mechanisms involved in dark-adaptation and night vision may be affected. These may be considered under two headings, photochemical and neural. Any photochemical changes that occur must occur in the retina, but neural changes could occur either in the retina or at higher levels of the visual system. The possibility of retinal changes will be considered first, for available evidence indicates that the retina is the locus of the dark-adaptation process. Thus, Craik and Vernon (19) found that the dark-adaptation curve obtained from an eye which was temporarily blinded by applying pressure to the sclera during light-adaptation was essentially normal as regards shape and level. As the primary stimulation was prevented from reaching the higher visual centres, their results indicate that adaptation is primarily a retinal phenomenon, although the matter is by no means conclusively settled (see for instance (62)).

1. *Sensory Mechanisms*

That photochemical changes occur during dark-adaptation has long been accepted, and some investigators (e.g. 84) still maintain that such changes are the *only* ones that occur. Briefly, photochemical theory claims that during light-adaptation rhodopsin and other visual pigments are bleached with the formation of products that are insensitive to light. As a result of the bleaching process the concentration of sensitive material in the retina is lowered and in consequence the visual mechanism becomes less sensitive to light. When the eye is put in darkness a resynthesis of the sensitive material from its photo-products occurs and sensitivity is recovered. In terms of this theory, the absolute light threshold is conceived as some simple function of the concentration of visual purple and other sensitive material in the retina.

Is it possible that in psychiatric patients the regeneration process is disturbed with consequent reduction in sensitivity? Examination of the individual dark-adaptation curves of neurotics in two of Granger's (39, 41) studies reveals a few cases (all hysterics) where the shape and level of the "rod" portion of the adaptation curve correspond fairly closely to the type of curve that has been obtained from subjects suffering from vitamin A deficiency. (As is well known, this vitamin is a constituent part of the photosensitive pigment or pigments.) Although there is no evidence of any marked deficiency in vitamin A intake among psychiatric patients, it is not improbable that some patients may suffer from conditions which can lead to a deficiency. Among the conditions which can

* Anomalies of the dioptric apparatus of the eye, such as refractive errors and opacities of the eye media may also have to be taken into account. Craik and Vernon (20) found, for instance, an association between a high perceptual threshold and high refractive error in their study of dark-adaptation.

either prevent or hinder the proper absorption or storage of vitamin A (110) are elevated metabolic rates, disturbances of the gastro-intestinal tract and others reported to occur as somatic features of neurosis and psychosis (2a). However, at least two factors make it appear unlikely that a photochemical explanation would have very wide application. First, recent evidence indicates that unless previous light-adaptation has been to a high intensity photochemical changes play a relatively insignificant part in determining the course of subsequent dark-adaptation. At ordinary room illuminations Rushton (102) could detect no appreciable change in the level of rhodopsin in the human retina, so it seems unlikely that photochemical effects would determine the results of most of the experiments reviewed above. With only one or two exceptions (39, 56) pre-adapting luminances have been comparatively low (below 300 foot L., the intensity used in Rushton's experiments) although admittedly the duration of light-adaptation has been for several minutes. In one experiment (39) light-adaptation was to an intensity of 750 ml. for five minutes and in another (56) to 1,720 mL for three minutes, and it is possible that photochemical changes played a significant role in both instances. However, Granger observed high thresholds in hysterics in a study using an adapting intensity of only 270 m.L. viewed for five minutes, and under these conditions a photochemical explanation seems inapplicable. In the second place, although psychiatric textbooks list a number of somatic features of neurosis that might hinder the proper absorption of vitamin A, there is no experimental evidence (2a) to show that these tend to occur more frequently in patients with high light thresholds (i.e. hysterics) than in other types of psychiatric patient.

Although an explanation along photochemical lines seems unlikely to have a very wide application, more detailed investigation of observed anomalies of dark-adaptation in specific cases might reveal significant differences due to interference with photochemical mechanisms. There is no doubt at all that regeneration of visual purple occurs after the eye has been exposed to the more intense illuminations (Tansley (115); Rushton (99)), and there is considerable evidence to show that patients suffering from vitamin A deficiency have impaired dark-adaptation (49), so it seems necessary to first exclude the possible effect of photochemical factors before considering alternative explanations in terms of nervous mechanisms. Rushton's (101) ingenious technique for measuring the rhodopsin level in the human eye should be useful in this connection.

Having considered the photochemical aspect of dark-adaptation, we will next consider neural mechanisms. Objections to a purely photochemical theory of dark-adaptation have been advanced by Lythgoe (70), Granit (44), Elsberg and Spotnitz (26), Thomson (117), Rushton (100), Baumgardt (7), and others, one of the most telling objections being that the rhodopsin concentration in the retina at a given time during dark-adaptation does not correspond to the visual sensitivity.

It now appears that in addition to any photochemical changes that occur, changes must also occur in the nervous apparatus of the retina. The exact nature of the mechanism involved remains to be determined, but interesting suggestions concerning its nature have been made by Lythgoe (70). This author was impressed by the fact that during dark-adaptation light sensitivity improves, but at the same time the finer visual judgments are impaired. This he suggests may be due to a re-organization of the neural connections in the retina, so that each fibre serves several elements by a spread of its synaptic connections: in other words, there is an increasing amount of convergence which makes possible the integration of feeble light stimuli, but at the same time there is a reduction

in the eye's capacity for the finer discriminatory reactions, for such reactions depend on the segregation of individual pathways. In other words, Lythgoe suggests that a type of "synaptic switching" occurs which has the effect of changing the properties of the retina from *differentiating* to *integrative*.

A similar idea has been put forward by Adrian and other authors. Adrian considers that "... the interaction of nerve cells in the retina may change in degree or in sign, darkness favouring mutual summations over wider and wider areas and light converting the effect to inhibition" (1, p. 110). Adrian goes on to say that it may be that "... as the light increases the sheets of nerve cells no longer act as a general collecting net for signals over a wide area, but begin to develop the local peaks and troughs of activity which make a detailed visual pattern" (1, p. 111).

While there is no direct evidence for the Lythgoe-type of theory, it seems to accord fairly well with electrophysiological as well as sensory data. Thus it has been shown that the size of the receptive field of a retinal element decreases with light-adaptation but increases with dark-adaptation (63, 64) while the summation area (15), and probably the summation time (100), and integrative ability of the eye (3), as measured by psychophysical methods, increase with dark-adaptation. Lythgoe's theory also seems broadly consistent with sensory work on the effects of light- and dark-adaptation on visual acuity and C.F.F. (72) as well as with Granit's studies of changes in the electroretinogram in the light- and dark-adapted retina of mixed type (i.e. containing both rods and cones). On the basis of Granit's studies it appears that "... a cone system is a mechanism for differentiation, a rod system one for integration. The properties of a rod system do not differ very much from those of an isolated receptor. The extensive convergence of a large number of receptors on to one final common path seems to be the main feature of the organization of a rod system, and this is of obvious importance in the integration of the feeble light stimuli characteristic of the scotopic eye. The cone system, on the other hand, seems to be organized for the interpretation of *changes* in the visual field, changes of illumination, of the area stimulated, of 'locus' in the field as a whole and ... of colour" (42, p. 167). Elsewhere Granit writes that the rod-dominated retina is "highly sensitive and slow like a ballistically recording galvanometer, integrating the total amount of light reaching it", whereas the light-adapted eye is much faster though less sensitive.

Both Lythgoe and Adrian claim that light-adaptation gives rise to some sort of "inhibitory" effect, whereby the spread of impulses arising from individual receptors is prevented so that they tend to work more as individual units. When the eye is put in darkness this "inhibition" presumably "dissipates" to be replaced by summative interaction in the retina. On this view light sensitivity and visual acuity are regarded as in a certain sense antagonistic functions, conditions favouring the one tending to militate against the other.

In proposing his model Lythgoe refers to the work of Schouten (103) on the so-called alpha-adaptation effect, a very rapidly occurring "inhibitory" effect which occurs when a glare source is applied to the eye. The effect of the glare source in depressing sensitivity is not confined to the specific area stimulated, but "spreads" to adjacent areas of the retina. It is possible that alpha-adaptation may operate at the fovea to prevent the spread of impulses from individual cones, thus ensuring the segregation of individual pathways and a high degree of visual acuity. In Lythgoe's view the uniformly-illuminated field used for light-adapting the retina before a dark-adaptation experiment may be regarded as composed of a multitude of contiguous glare sources. On this

theory, the subsequent dark-adaptation curve would represent to some extent a recovery from inhibition (52).

Even if Lythgoe's account of the nature of the changes in retinal *organization* should prove to be grossly inaccurate and be replaced by an alternative explanation such as that of Pirenne (89, 90), there can be little doubt on sensory and electrophysiological grounds that changes in retinal *properties* occur under conditions of light- and dark-adaptation, the light-adapted eye *apparently* making greater use of inhibitory mechanisms than the dark-adapted eye which seems to depend more on summative processes. Accepting such a change in what may be termed the "excitation-inhibition balance" of the eye, the possibility may be considered that some of the anomalous dark-adaptation curves observed in psychiatric patients may be due to some disruption of this balance.

The raised light thresholds of hysterics and their slower recovery from light-adaptation might result from an exaggeration of inhibitory and a weakening of summative processes. On this hypothesis the hysterics would be expected to behave like normal subjects whose eyes had been exposed to a light-adapting field or "glare source" of greater intensity, or to one of the same intensity acting for a longer time (46). The greater "inhibitory" effect of light in the hysteric would show itself in the modification of such features of the adaptation curve as the initial threshold value, the slope, the time of the cone-rod transition point and the time course of subsequent adaptation. A weakening of summative mechanisms would show itself by reducing the summation area of the retina, i.e. the hysteric would behave like a normal subject confronted by a test-field of reduced size, with consequent upward displacement of the dark-adaptation function along the log-luminance axis and shallower slope. Experimental data are not available on all these "deductions", but existing data tend to be consistent with the type of hypothesis proposed, so far as the initial threshold value is concerned. Also consistent is Gravely's (45) finding* of a higher C.F.F. in hysterics than in anxiety states.

In anxiety states apparently the summative mechanisms are unimpaired or even enhanced during dark-adaptation so as to produce normal or lower than normal thresholds, but the integrative ability of the retina is perhaps achieved at the expense of differentiation due to impairment of lateral inhibitory processes. Although able to integrate feeble light stimuli due to summative interaction between adjacent retinal areas, the retina of the anxiety state may be unable to "inhibit" the spread of impulses sufficiently to enable him to appreciate the contour or detail of test-objects. In consequence, although his eye would readily detect the presence of light in the visual field, in such a test as the Livingston Rotating Hexagon the outlines of objects would tend to merge into the background and appear blurred and indistinct. Such a process would be expected to operate in the perception of test-objects of fairly complex contour at the higher "photopic" levels of illumination covered by this test, but it seems less likely to play a part in perception of the simple geometrical test objects in the adaptometer tests at the lower intensities.

In addition to possible interference with the retinal mechanism of dark-adaptation, the possibility should also be considered that impairment may occur in the transmission of impulses along the conducting pathways between the retina and cortex while the receiving areas in the visual cortex itself might be affected. Even the staunchest proponents of photochemical theory, such as Hecht and his co-workers, have never denied that the state of the nervous system beyond the photoreceptors can affect the value obtained for the threshold

* Not statistically significant.

energy during the course of dark-adaptation. (What they do deny is that sensitivity changes can occur in the nervous system *in response to light*.) Any factor which interferes with the transmission of impulses from the retina would mean that the light energy required to produce a threshold sensation would differ from the normal value and might result in the dark-adaptation curve being shifted along the intensity axis.

Assuming that neural effects occur in the receptors, retinal synapses, optic nerve fibres, lateral geniculate bodies, or at higher levels of the visual mechanism, the problem arises as to the factors underlying such effects. How could anxiety, hysteria and other psychiatric conditions affect the neural mechanisms involved in night vision? Here unfortunately there is hardly any information that bears on the problem either from psychiatric or from visual research so that only a few tentative suggestions can be made. One possible lead comes from studies of the effects of drugs and physiological stresses on visual thresholds. McFarland has shown for instance that anoxia (75) has the effect of elevating the dark-adaptation curve along the log-luminance axis by amounts varying between about 0.2 and 0.4 log unit, without changing its shape. This effect seems to be due not to changes in the photochemical system of the retina, but rather to alterations in the central nervous system. It is well known that nervous tissue is particularly sensitive to a deficit of oxygen, and it is quite possible in view of the close relationship between the retina and the brain, embryologically and physiologically, that changes may occur in the retina itself, although this cannot be established from McFarland's findings. However, there can be little doubt that there is a depression of neural activity somewhere in the visual system.

Similar results have been obtained from subjects suffering from insulin hypoglycaemia (77). When the blood-sugar level is lowered by injecting insulin the dark-adaptation curve becomes elevated by amounts varying between 0.1 and 0.4 log unit. Return to normal follows either from the inhalation of pure oxygen or from the administration of dextrose. Elsewhere McFarland and Forbes (76a) have shown that hyperglycaemia produced by the injection of dextrose can partially counteract the effects of anoxaemia on light thresholds. It appears from these results, as McFarland (76a) has pointed out, that oxygen deprivation and hypoglycaemia have similar and additive effects on dark-adaptation. Raised light thresholds have also been reported in subjects under the influence of alcohol (78) the effect being attributable presumably to depression of central nervous activity.

The possible bearing of these studies on our problem comes from the fact that similar upward displacement of the dark-adaptation curve is found in psychiatric groups, and it may be that some of the same mechanisms are involved. Several studies have suggested the presence of oxygen deficit in schizophrenics (73) while Gellhorn *et al.* (33) have reported more insulin under stress conditions in psychotics than in normal subjects. It is possible that such factors may be partly responsible for the raised intensity thresholds found in Marshall and Day's (81) and Granger's (38) studies. Such factors seem less likely to operate in neurotic groups, although McFarland (74) found that neurotic patients of the "chronically fatigued type" had a lower "altitude tolerance" than normal subjects: a large percentage of both male and female neurotics either collapsed or approached collapse at simulated altitudes of 14,500 feet (12 per cent. oxygen) and 19,000 feet (10 per cent. oxygen) than did normal subjects.

Perhaps significant also is the fact that hysterics who show raised light thresholds and behave like normal subjects under the influence of central

nervous system depressants, appear to have lower than normal sedation thresholds for sodium amytal (108) as determined by Shagass' (107) index of sedation tolerance. To use Shagass's terminology, his results suggest a lowered "cerebral excitability" in hysterics as a group, and particularly in hysterics showing conversion symptoms. The index which Shagass uses is based on certain quantitative EEG changes associated with the onset of slurred speech. The EEG changes are recorded from transverse frontal electrodes. Whether the lowered "excitability" extends to the visual cortex or other parts of the visual system is unfortunately not known.

A further observation that seems worth noting in connection with depressant drugs is Trent's (118) finding of enhanced contour acuity following the administration of Nembutal. This result suggests that Nembutal may have some effect on lateral synaptic connections in the visual system, enhancing form acuity by reducing summative interaction between adjacent areas. The effect of Nembutal on contour acuity may be analogous to that of light-adaptation or a "glare source" applied at some distance from the test area (103), acuity being increased by a reduction in light sensitivity. If Nembutal were shown to have a depressing effect on the light sense its action might parallel that of hysteria in a test of the Rotating Hexagon type involving form discrimination at low (photopic) intensities.

The depressing effect of Nembutal may be contrasted with the facilitating effect of strychnine which, from electrophysiological as well as psychophysical studies (2, 43) is known to increase areal interaction in the retina. It has also been shown to lower light thresholds and improve dark-adaptation (34)* and although part of this effect may be due to increased excitability of the conducting pathways between the retina and the brain, the above studies and others (35) suggest that strychnine may have a local effect upon the retinal synapses, and increase the summative ability of the retina. Therman's studies (116) have shown that the application of strychnine tends to enhance the PII component of the electroretinogram which is associated with excitation in the optic nerve. Unfortunately, as in the case of Nembutal, the relative effect of strychnine upon the light and form senses is not known. Its ability to increase the degree of interaction between adjacent areas of the retina would suggest that it may enhance the summative and integrative ability of the eye at some expense to differentiation and acuity. If so, it might be tempting to consider its effect as analogous to that of anxiety, although here we are going somewhat beyond the experimental evidence. It will be remembered that anxiety states had light thresholds that did not differ significantly from normals, whereas strychnine *improves* light thresholds. However, a tendency was noted particularly over the last twenty minutes or so of dark-adaptation for anxiety states to have lower thresholds than normal. This tendency would probably become significant if a normal group strictly comparable in age to the neurotic group were used. If this were the case, it might be possible by means of drug studies to relate these visual effects of anxiety to Shagass's (108) finding of increased "cerebral excitability" in anxiety states, as determined by their "sedation threshold" for sodium amytal.

Another line of approach comes from studies on the so-called "adaptive influence" of the sympathetic nervous system on the "visual analyser", following Orbeli's (86) theory. Babskii (4, 5) has shown, for instance, that adrenalin tends to lower light thresholds and improve dark-adaptation, while sympathectomy impairs these functions. The effects appear to be independent of changes in

* Some investigators (78, 97), however, report no effect.

pupil size. These results may be considered in relation to the view often put forward by clinicians that manifest anxiety tends to be reflected in increased sympathetic activity and also to Cannon's well-known theory of the role of the sympathico-adrenal system in emergency (fear) responses. Looked at from a biological viewpoint it is possible that a fear reaction might have as part of its effect the enhancement of the more primitive light sense at the expense of the more recently acquired form sense. The fear response might be triggered off by darkness acting as a kind of conditioned "stimulus" in anxiety states. Perhaps worth noting is the fact that anxiety states tend to form conditioned reflexes readily (30). Here again it would first be necessary to demonstrate conclusively that anxiety can actually *improve* light sensitivity before going on to test further hypotheses.

Somewhat difficult to reconcile with Babskii's (4) results on the beneficial effects of adrenalin on dark-adaptation are Therman's (116) electrophysiological results, which show that adrenalin tends to depress the b-wave of the E.R.G. and optic nerve discharge and Marazzi's (79, 80) work on the inhibitory effects of adrenalin on the visual system. The effects of adrenalin on the E.R.G. are very remarkable, although very complex. Its most pronounced effect is probably that of slowing down all the reactions and producing an enormous prolongation of the latent period (116). The depressor action of adrenalin on the retina may be compared with results obtained elsewhere in the C.N.S. (105) where from time to time increased excitability also occurred. Therman's remarks that adrenalin ". . . may have something to do with excitation in the retina . . ." are worth considering in relation to the Russian theories of the "adaptive influence" of the sympathetic nervous system on the visual mechanism. In some way adrenalin may affect the "excitation-inhibition" balance of the eye, but its effects seem likely to be rather transient. Momentary depressions of light sensitivity such as one might expect on the basis of the electrophysiological results might reveal themselves in a dark-adaptation curve,* provided thresholds were taken at sufficiently short intervals of time, by "plateaux" or periodic oscillations, although other explanations are possible (see below). Such oscillations have been reported by Lee (66), while Granger (39) found evidence of "plateaux" in the curves of anxiety states.

Relevant also to the discussion of sympathetic activity and anxiety are effects of breathing pattern upon the visual threshold. Increasing the rate of breathing room air by 50-100 per cent. is known to result in a fall in the absolute light threshold to about one-half its value within 5-10 minutes (120). This effect appears to be due to alkalosis associated with hyperventilation and is probably due to changes occurring in the visual system at some point or points central to the photoreceptors. Such an effect might occur as a consequence of increased sympathetic activity in anxiety states although it must be noted that increases in respiration rate have been found also in other types of neurotic (2a). Of possible significance also in connection with the distinction drawn earlier between the integrative and differentiating ability of the retina is the fact that rapid breathing has been found to *raise* the threshold for brightness discrimination in one study (32).

If anxiety involves both central and autonomic components, as has been suggested, a drug that would seem to be of particular interest for further research is ephedrine which is not only a central nervous stimulant but is also a potent sympathicomimetic agent which simulates in its peripheral actions

* Rothan (97a) found a decrease in the capacity of an eye for dark-adaptation following instillation of Adrenalin into the conjunctival sac.

results obtained by stimulating adrenergic nerves. Holzer (51) noted an improvement of dark-adaptation following administration of this drug but his findings have not been confirmed by other investigators (37, 97). Apparently discrepant findings are very common in experimental work on the effects of drugs on dark-adaptation (see for instance reviews by Rose and Schmidt (97) and Segal (106)) and this must be borne in mind in connection with the suggestions made in the preceding paragraphs. While some of the discrepancies are undoubtedly due to physiological factors, it seems likely that many are due to the use of different physical conditions of experimentation, threshold criteria and test-objects. For instance, it is possible that a given drug may have a stimulating effect on absolute light sensitivity, as measured by the "fixation-and-flash" method, and yet impair the eye's ability to perform the finer visual tasks. So far there has been no serious attempt to study the differential effects of drugs on specific dimensions of visual functions; such research is urgently needed. Also needed is research employing more satisfactory experimental designs and statistical techniques than have been used in some of the earlier studies.

Before concluding this section on sensory factors, reference should be made to Eysenck's (27) theory of anxiety and hysteria in which he claims that differences in conditioning, learning and various perceptual functions between these two clinical groups reflect differences along a personality dimension of "introversion-extraversion". Briefly Eysenck claims that introverts and extraverts and their neurotic counterparts, anxiety states and hysterics, differ with respect to the speed and strength with which *reactive inhibition* is generated and the speed with which it is dissipated. Following Hull (53) Eysenck regards reactive inhibition as a molar property of the central nervous system. Individual differences in the development of reactive inhibition Eysenck regards as largely due to heredity. So far Eysenck's theory has been developed mainly in relation to the variables of learning experiments (28, 30) but it seems likely that, given a more molecular formulation, it may be applicable to certain psychophysical data from visual experiments. This possibility will be considered in some detail in another paper. Suffice it here to say that the recovery of sensitivity following light-adaptation (i.e. a period of continuous stimulation) may represent in part a recovery from "reactive inhibition". On Eysenck's hypothesis one might expect (other things being equal) recovery to be more rapid in anxiety states than in hysterics while an unselected group of normal subjects would fall about midway between the two clinical groups. While it is difficult, owing to lack of data, to accurately locate the position of normals from Granger's (41) study, it is worth noting that anxiety states had more rapid recovery times than hysterics, as determined by their initial perception times.

2. Motor Mechanisms

Seeing under conditions of low illumination does not only involve sensory mechanisms, and one must consider possible effects of psychiatric illness on threshold responses to be mediated through the intra- and extra-ocular muscles of the eye. Pupil size is of obvious importance in that it governs the amount of light entering the eye at any given time. An unusually constricted pupil during dark-adaptation tends to raise the light threshold, whereas a very dilated pupil tends to lower it (87). Again, a large pupil during light-adaptation prior to the dark-adaptation experiment tends to increase the time taken to perceive the initial test-field and affect the early course of dark-adaptation. On the other hand, a small pupil has the opposite effect.

Pupillary reactions at the time of change-over from light to dark-adaptation

may also have important effects upon the threshold. During the first few minutes of dark-adaptation the pupil relaxes allowing more light to enter the eye and thus permitting progressively lower intensities to be perceived. As a consequence of changing pupillary diameter the speed of the first part of the "cone-adaptation" curve may be increased and the total range of adaptation increased slightly by an amount equal to the extent of pupillary dilation. Not only would anomalies of pupillary dilation exert some effect upon the adaptation curve, but spasms in the form of sudden constrictions, which are known to occur in some subjects (21), would have at least a momentary effect upon the light threshold. Although there is much suggestive clinical material (Granger (36)) relating anomalies of pupil size and pupillary light reactions to neurotic and psychotic disorders, there is unfortunately no experimental evidence* directly relevant to dark-adaptation. DeJong (23) claims that in the so-called vagotonic individual with cold skin, bradycardia and low blood pressure the pupils are contracted owing to overactivity of the parasympathetic, whereas in the sympathicotonic individual with warm skin, rapid pulse, and hypertension there is mydriasis. Anxiety states and certain schizophrenics are classified by DeJong in the second group. The consequences of a larger than normal pupil size would depend on whether the eye were light-adapting or dark-adapting. A large pupil during light-adaptation would delay recovery in the early stages of dark-adaptation but would tend to lower the light threshold later on. Duke-Elder (25) has noted that miosis can occur in hysterics, but here again the consequences for the dark-adaptation situation are difficult to predict, although a positive correlation has been found between pupil size in light and in darkness for *normal* subjects (9). Much depends on whether the miosis occurs under dark- as well as light-adaptation. It is possible that hysterics may have smaller than average pupils during dark-adaptation due to the longer persistence of light-adaptation effects in such subjects, for Thomson (117) has shown that the light-history of the eye for some time before a dark-adaptation experiment can affect pupil size (smaller pupil) even though the eye is in darkness, the effect being mediated presumably by off-fibres.

Changes in accommodation could possibly affect the experimental results either directly by causing blurring of vision or indirectly by affecting the pupil size. While it is difficult to see how accommodation could be a significant factor in affecting the perception of large test-fields at scotopic intensities, it is possible that accommodation might enter as a factor at the photopic intensities used in the Livingston Rotating hexagon test for Campbell (16) has shown that the accommodation reflex is activated when the light energy of the test-field exceeds about 1 mL for a 1° test-object. This luminance level corresponds to the lower limit of foveal vision (i.e. to the higher intensities used in the Livingston test). Campbell's results indicate that the threshold for the accommodation reflex is between 0.25 and 0.5 $\log_{10}$ unit higher than the visibility threshold. Under the conditions of Rees's (93) and Livingston and Bolton's (67) experiments where relatively small test-objects are viewed at short distances (1 yard) for a period of one minute, accommodation changes might affect perception.

Changes in accommodation would also affect the pupil size, and Ditchburn and Steele (24) have pointed to the necessity for ensuring that subjects hold the fixation spot in dark-adaptation studies not only as regards the general direction of viewing, but also as regards focus. Accommodation changes could play a part not only during dark-adaptation but also during light-adaptation by

* Unpublished data of Granger provide evidence of a significantly smaller pupil size (light-adapted eye) in psychiatric patients than in normal subjects.

affecting pupil size, if any stimulus were present to induce accommodation. Under the light-adaptation conditions of most of the experiments reported above there is no obvious stimulus to accommodation except in the case of one of Granger's (39) experiments where variations in texture of the hemispherical "bleaching bowl" may have caused some subjects, in spite of instructions to the contrary, to accommodate from time to time, with consequent change in pupil size.

Unfortunately, little is known about anomalies of accommodation in psychiatric patients, although they have been observed by Granger (29) in another context. It is interesting to note that any excessive sympathetic activity occurring in anxiety would tend to affect the accommodation mechanism as well as the pupil size and reactivity. Relaxed accommodation in tests involving form discrimination (93) might interfere with clear perception of the test-object at photopic intensities. Spasm of accommodation has been reported in hysteria first by Charcot and Galezowsky (18) and since by Borel (13), Morax (83), Plantegna (92) and Shastid (109), often in association with convergence spasm and miosis. In this condition the tone of the ciliary muscle is increased and has been regarded as due to hyperactivity of the parasympathetic nervous system, although hypoactivity of the sympathetic may also enter as a determinant.

Motor disturbances in the form of oculo-motor imbalance which appear to occur more frequently among psychiatric patients than among normal subjects (36, 29) might play some part in tests involving binocular vision. In dim illumination where the stimulus to fusion is low it is possible that binocular vision may not be attained so that no binocular summation (71) occurs to lower the threshold. Alternatively, the chances of the eyes receiving sufficient light quanta to elicit a visual sensation would be reduced owing to reduction in area of the receptor surface (Pirenne (88)).

Finally, eye movements associated with fixation could affect threshold responses depending on whether the light stimulus fell on a more or on a less sensitive area of the retina. In addition, very precise fixation could actually result in impaired sensitivity in a subject in a test in which a test-object was presented for several seconds with good light sensitivity, but relatively poor form perception. Accurate fixation would mean that a patch of light of supra-threshold intensity continued to stimulate more or less the same retinal area for a matter of seconds. After a period of approximately 5-10 seconds the brightness of the light stimulus would tend to diminish and eventually disappear altogether owing to "local adaptation" (see, for instance, Pirenne *et al.* (91)).

3. *Perceptual Factors*

In addition to effects on the sensory and motor mechanisms of vision it is possible that effects may occur also at the "higher" perceptual level. The hypothesis should be considered that differences in "night vision" between normal and psychiatric groups are *entirely* due to factors involved in the process of attending to the stimulus and making threshold judgments under conditions of low illumination rather than to differences of a sensory or motor nature. Many writers (8, 20, 122) have stressed the importance of such factors although relatively little experimental work has been done to determine their precise significance.

In subjective researches on "night vision" a subject's attention has to be directed to the stimulus by means of instructions from the experimenter. He must be instructed on "how to look", what criteria to use in making judgments, to react immediately he sees the test-object, etc. Learning how to look for objects

under conditions of low illumination is a skill that may be acquired more readily by some subjects than by others, and those who acquire it readily will in consequence tend to see the test-object sooner and have lower thresholds, because they are quickly able to locate the stimulus on the most sensitive retinal area. Such a skill may be of importance in tests allowing unrestricted viewing of the test-field such as have been used by Rees (93), Livingston and Bolton (67), Gravelly (45) and Granger (38, 39).

In several of the above experiments some attempt has been made to instruct the subjects on the best viewing procedures but in no case was there any special period of training involved and it is conceivable that individual differences in threshold may simply be due to the fact that individuals were not all looking in the right direction at the right time. However, several considerations make it seem unlikely that such an hypothesis is a probable one. In the first place, under the stimulus of light the eye tends, to some extent, to adopt a position whereby the most sensitive region of the retina is stimulated. In the second place, admitting that a certain skill must be developed in viewing the test-object, there is no evidence outside the "night-vision" experiments to suggest that psychiatric patients will in general develop this skill less readily than normal subjects. Third, if learning were a significant factor in accounting for the differences between normals and patients it might be expected that after the first few judgments in the early stages of dark-adaptation the dark-adaptation curves of normals and neurotics would converge; such is not the case however. Fourth, if it were argued that psychiatric patients as a group do not pay attention to the task as well as normal subjects, it would be difficult to explain their immediate reactions to sudden changes deliberately introduced into the stimulus situation in one of Granger's (41) experiments to check the vigilance of the subjects. Further, it would be difficult to argue on the basis of clinical observations that certain types of patient, for instance hysterics, would tend to pay less attention than anxiety states and therefore have higher thresholds in tests of night vision, for in Rees's experiment hysterics tended to have lower thresholds.

Nevertheless, even though hypotheses such as those considered above seem unlikely to have much *general* application to psychiatric groups it is conceivable that apparent anomalies shown by certain patients may be due to lack of training in peripheral viewing and it may be significant that in an experiment in which, according to one of the authors,* much time was spent in giving information about the nature of dark-adaptation and the importance of peripheral viewing, differences between neurotics and normals were insignificant. Certainly, at least so far as peripheral acuity is concerned, training procedures have been shown to produce marked improvement (68, 69).

As regards the variation to be expected from the use of different subjective criteria of perception, Hunt and Palmer (54) have shown that this can amount to as much as 0.5 log unit. They investigated the various stages of perceptibility of test-objects following thirty minutes dark-adaptation and found a mean threshold of 3.3 log unit for "bright image with form" compared with a mean threshold of 2.8 log unit for a just perceptible light with form absent. It is quite possible therefore that part or all of the differences observed between normal and psychiatric groups is due to the use of different criteria by the two groups. However, it seems unlikely that such a factor could account for all the experimental results. If it were argued (e.g. in (20)) that anxiety has the effect of making patients hesitate and delay making a judgment on the basis of their initial impression of light, why is it that anxiety states behave essentially like

* Personal communication from Dr. M. Desai.

normal subjects in Granger's (41) study? And if it were argued that hysterics in view of their personality characteristics would tend to be impulsive and reckless in making judgments, responding very rapidly to the first impression of light rather than waiting to be sure, why did they show raised thresholds in Granger's study? In spite of its apparently limited application in accounting for experimental results this "criterion" hypothesis may nevertheless be applicable in certain cases and cannot be rejected out of hand at the present stage of experimentation.

Other factors that might have to be considered include reaction time and the "meaning" visual stimuli have for particular patients. Even if a subject adopts the correct subjective criterion in making a threshold judgment, his response time in reporting upon his sensation would enter as a factor in determining his threshold value. It has been demonstrated that the reaction times of psychotic patients tend to be longer than those of normal subjects under a number of different experimental conditions (36) and it is possible therefore that this may be a factor affecting threshold measurements in night vision tests.

The problem of "meaning" is a very complex one. It is mentioned because Janda (56) has suggested that anxiety may have the greatest influence upon perception when meaningful complex forms have to be identified. If this were so, it might be that in Rees's experiment the factor responsible for the inferior performances of anxiety states was associated with the meaningfulness of the stimuli. That, in other words, the significant feature of the task was that it demanded form *identification* rather than merely form *discrimination*. However, in spite of Janda's suggestion, it is still difficult to see precisely how such a factor would operate. Should "meaning" prove to be important in subsequent analysis it would seem that the services of a learning theorist might be needed here as well as in analysing the more general aspects of the "dark vision" situation (e.g. darkness serving as a conditioned stimulus to a fear response).

Discussion of the possible influence of meaning leads one to a consideration of the work of Klein (60), Frenkel-Brunswick (31) and others, on the effect of motivational factors in perception and to such concepts as that of "intolerance of ambiguity". According to Klein, individuals differ in their degree of tolerance of ambiguous perceptual situations. On this type of hypothesis it might be argued (see for instance (56)) that a test of "night vision" such as Rees used is an ambiguous situation in that the various stimulus shapes are only dimly seen and lack definite structure. In such a situation hysterics (who are supposed to repress anxiety) would *need* to impose a structure and remove ambiguity sooner than would anxiety states who could tolerate the ambiguity for a longer time. However, although such a "mechanism" might cause hysterics to respond sooner than anxiety states, there seems no reason why their responses should be more correct. Hypotheses of the Kleinian type have been given much prominence in the "personality via perception" movement and probably deserve some consideration, but it would seem necessary in all situations involving sensory factors to exclude their effects first before postulating differences at higher levels. While perceptual factors of the type discussed could operate independently of sensory and motor factors interaction is also possible. For instance, hesitancy in reporting upon the presence of light might in turn lead to a "local adaptation" effect resulting in further increase in the value of the threshold energy.

V. IMPLICATIONS FOR FURTHER RESEARCH

It appears from the above discussion that psychiatric disorders may affect visual functioning at one or more of three different levels, sensory, motor and

perceptual. All three types of factor probably enter as determinants of threshold responses in most of the studies reviewed, the relative involvement of the three types varying with different experimental conditions. Thus, perceptual factors seem likely to be more important in the Livingston Hexagon test than in Granger's (41) study of light thresholds.

To determine the relative importance of the three types of factor, future studies will have to be more analytical than some of those undertaken hitherto; in particular the psychometric studies need to be supplemented by psychophysical and psychophysiological experiments. This means that physical, physiological and psychological controls must be stricter if ambiguity is to be avoided in the interpretation of results. In this section a few general comments will be made concerning the implications for further research and type of experimental technique that seems to be demanded.

To prove the existence of physiological or psychological differences between normal subjects and psychiatric patients great care must be taken in the control and specification of physical variables in the stimulus situation, such as the previous level of light-adaptation, retinal area stimulated, duration, intensity and colour of stimulus object, etc. The importance of excluding variation due to physical factors cannot be too strongly emphasized at a time when there is a tendency to seize upon any observed variation between individuals as due to personality characteristics of a fairly permanent nature (e.g. the "personality via perception" approach (12)).

To prove the existence of sensory differences fairly lengthy periods of training may be necessary in order that the subject shall look in the right direction at the right time and in the right way. In other words, it may be necessary to make psychiatric patients as "sophisticated" as the observers used in psychophysical research on vision. It may also be necessary to use only certain psychophysical methods for obtaining threshold responses for Blackwell (11) has shown that some methods more than others allow processes other than "sensory excitation" to enter as determinants of threshold responses. Further, subjective techniques need supplementing by objective methods. It would be highly interesting, for instance, to compare the dark-adaptation curves obtained using sensory methods with those obtained using the electroretinogram (57, 58, 59, 94) or optokinetic nystagmus technique (17, 104) when these have been further developed.

To determine the significance of photochemical as contrasted with neural mechanisms Rushton's (101) optical technique could possibly be used, while the effect of extra-retinal factors could be determined by comparing dark-adaptation curves following binocular and monocular light-adaptation (6). Craik and Vernon's (19) "blinding" technique, previously referred to, although valuable for use with co-operative normal subjects could scarcely be applied to psychiatric patients.

In all studies more attention must be given to possible influence of drugs received as part of a patient's therapy. For instance, patients receiving insulin therapy may have elevated light thresholds due to insulin hypoglycaemia (77) rather than to psychiatric disorders as such. Little is known about the effect of barbiturates on dark-adaptation, but from what is known about their effects on related visual functions such as C.F.F. careful control of such drugs would seem to be demanded.

Assessment of effects due to motor mechanisms of the eye may be made by objective techniques. Pupil size and pupillary light reactions can be recorded in darkness using either flash photography or infra-red cine-photography.

Reasonably effective techniques have been developed for this purpose for use with trained observers although their application may prove somewhat difficult in the case of certain psychiatric patients. Elimination of effects due to pupil size can (in principle) be accomplished quite simply by placing an artificial pupil in front of the subject's eye or by using an optical system employing Maxwellian view. Here again, practical difficulties may arise in applying such techniques to psychiatric patients owing to the difficulty of centring the eye pupil relative to the apparatus. Should the recording of changes in accommodation during dark-adaptation become necessary Campbell's (16) technique would seem to be applicable. Campbell has determined accommodation changes by photographing the third Purkinje-Sanson image formed by reflection from the anterior surface of the lens; the size of this image depends on the radius of curvature and hence on the state of accommodation of the eye. For the recording of eye movements sensitive techniques are available (95) that could be adapted to the requirements of dark-adaptation experiments. Of particular interest would be the measurement of eye movements during fixation.

The influence of perceptual factors could be determined by a process of elimination of sensory and motor factors along the lines suggested above, although in order to distinguish between different types of perceptual factor it would be necessary to devise special experiments. The effect of "meaning" could for instance be investigated by comparing the effects of abstract stimuli with "meaningful" stimuli of the same visual acuity value.

As regards the type of experimental design required for further experimentation, the factorial type of experiment would seem to be necessary to determine under given experimental conditions the relative importance of different types of factor and interactions between them. In certain cases, however, the traditional psychophysical type of experiment would seem to be necessary for the detailed analysis of the effects of a single factor. As regards the analysis of experimental data, as these would take the form of adaptation *curves* rather than single "scores", methods of curve fitting would seem to be demanded. Hammond and Lee (47) have developed a useful technique for the treatment of data in individual dark-adaptation curves which depends on fitting an equation to the "rod" portion of the curve (obtained using the Hecht-Shlaer procedure).

Analytical studies of the type suggested could be undertaken with various psychiatric groups or with individual patients. Choice between the two approaches could be made only in the light of experimental results. Where evidence indicates that certain psychiatric categories show a fair amount of homogeneity in their visual reactions it would seem profitable to study the group as a whole. In other instances the study of sub-groups or particular individuals may be more appropriate. Intensive studies of relatively few individuals would seem to be the only practical possibility if experimental controls are to be rigorous. In this case the pattern of experimentation would follow that of visual psychophysics in which for the most part detailed measurements are made on only one or two observers. Such studies seem likely to prove more valuable in eliciting psychophysiological mechanisms than more cursory surveys of larger groups, although large-scale surveys may have their place in further psychometric development of a given "test".

As regards the most appropriate starting-point for further research, choice must depend largely on the viewpoint of the investigator and the writer can only indicate his own proposals. In his view, one of the most interesting problems raised by previous research is that of the possible differential effects of anxiety and hysteria on the light and form thresholds. Attacks could be made on this

problem from several angles. Thus, recovery curves of hysterics following light-adaptation would be expected to differ in certain features (see earlier discussion) from those of normal subjects and anxiety states. Differences in integrative ability could be determined using test-fields of different sizes and calculating an index of integration (3) from the threshold differences between smaller and larger fields, while comparison of thresholds for simple light stimuli with those for complex forms of varying acuity values would provide information on the postulated differences between anxiety states and hysterics. Studies of this type could be supplemented by studies attempting to produce differential effects on light and form sensitivity by means of drugs similar to those produced by psychiatric disorders.

Before proceeding very far with such studies it would, however, be necessary to determine more precisely the position of anxiety states' dark-adaptation curves relative to those of normal subjects. It may be that it is only toward the final (rod) threshold that anxiety states differ from normal; the initial portion of their dark adaptation curves may follow the normal course. What is required initially here as elsewhere in this area of research are studies designed to specify more precisely the nature and extent of differences between psychiatric patients and normal subjects in terms of properties of the dark-adaptation curve such as initial threshold following light-adaptation, slope of both the cone and rod portions of the curve, cone-rod transition time and final threshold attained after a long stay in darkness. The results of such studies will almost certainly indicate the need for radical revision or rejection of some of the hypotheses suggested earlier in this paper to account for the experimental results so far obtained.

The future course and final outcome of experimentation in this relatively new field of research are impossible to predict but from the evidence reviewed here and from studies of critical flicker frequency (110a) and other visual functions (29) it seems likely that visual thresholds may prove valuable indicators of physiological imbalance occurring in psychiatric disorders. Compared with other possible measures visual thresholds have several features to commend them, as McFarland (76a) has pointed out. In the first place they appear to be particularly sensitive to stress conditions (for instance, changes in visual thresholds are among the first to appear in anoxia); second, the physical measurements involved can be made with considerable precision and data lend themselves to precise quantitative treatment; third, the control of experiments is simplified by the fact that the subject is unaware of changes in his sensitivity. "He does not know what changes in the physical intensity of the stimulus are necessary in order for him to see it, since at his threshold the stimulus always has the same appearance" (76a, p. 328). Temporary masking of impairment by exerting extra effort is impossible.

From the psychophysiological point of view the fact that the peripheral sense receptor in the case of vision is not only a sense organ in the strict sense of that term but is also part of the central nervous system gives to the investigation of visual dysfunctioning a possibly wider significance than merely that of studying disorders of a special sense. Investigations of visual thresholds become an avenue of approach to the central nervous system and its dysfunctions.

Although it is probably from this point of view that investigations of visual anomalies in psychiatric patients will chiefly be regarded, one should not overlook the fact that visual functions in which changes in the photochemical system of the retina play a significant role (e.g. the recovery of sensitivity in the dark following exposure to high intensities of illumination) may also serve as indices

of physiological imbalance. Disturbances of the gastro-intestinal tract, hepatic and circulatory disorders reported to occur in various psychiatric illnesses (2a) can affect the value of the threshold energy and the course of dark-adaptation by interfering with the absorption and storage of vitamin A. Associated with some of these and other physiological dysfunctions occurring in emotional disorders is the autonomic nervous system and here again the eye serves as a valuable indicator of autonomic activity either directly via the effector mechanisms controlling pupillary reactions and accommodation or indirectly through possible effects on the sensory mechanisms of vision.

The many possible avenues through which various types of physiological imbalance may be reflected in visual and ocular terms perhaps helps to explain why anomalies have been observed in so many different groups of psychiatric patients, showing marked heterogeneity of symptoms. This sensitivity of visual and ocular functions to dysfunctions occurring in a number of different bodily systems although in certain respects an advantage (e.g. in larger-scale psychiatric screening procedures) presents difficulties when interpreting results obtained from psychiatric groups for disorders of dark-adaptation, critical flicker frequency, etc., are not specifically related to any particular stress or abnormal condition. Depression of sensitivity may have a variety of causes and it seems probable that many of the causes operating to produce reduced sensitivity in psychiatric patients are the same factors that produce lowered sensitivity in organic diseases, such as cardiovascular, metabolic, hepatic diseases, etc. In view of this it seems necessary if we are to understand the mechanisms underlying the effects of psychiatric disorders on vision to consider their effects in relation to those of other pathological conditions.

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CRITIQUE OF PSYCHOLOGICAL APPROACHES TO BRAIN DAMAGE

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THIS paper will make an attempt to examine critically the major psychological approaches to the problem of brain damage and to point out why such a vast number of psychological studies of neurological patients have on the whole proved to be disappointing. Up to the present time most of the findings are equivocal, inconsistent, and very often contradictory; no acceptable theory of brain functioning and organization has emerged from them and the diagnostic tests in this field show too high a degree of misclassification to be sensitive enough for clinical use. Finally, an attempt will be made to outline a more fruitful approach to brain damage.

DEVELOPMENT OF PSYCHOLOGICAL APPROACH TO BRAIN DAMAGE

Since the very early observations made by Broca (13), and Seppilli (85) that disturbances of speech are related to lesions of the dominant hemisphere, neurologists have included various intellectual disturbances among the localizing signs of cerebral lesions. Since that time the relationship between lesions and intellectual impairments has remained a clinical problem.

Not until 1929, however, was an attempt made to *measure* the actual abilities of neurological patients, using psychological tests. Since then a great many studies have been reported in which more or less standardized psychological tests have been employed.

The main trends of such research may be briefly outlined. As Klebanoff (43) has pointed out, at the beginning psychological studies in this field primarily concentrated on correlations between mental functions and specific areas of brain lesions; the ultimate aim of these studies was a diagnostic one being mainly concerned with the definition of an "organic psychological syndrome". During the last decade research has been directed more to the developmental aspect of brain injury, patterns of functioning and the consequences of neurosurgical operations. Very recently several studies have appeared which, using analysis of variance, have concerned themselves with the possibility that lateral and lobar localization of lesions might be associated with differential test behaviour.

THEORIES OF BRAIN FUNCTIONING AND ORGANIZATION

Readers interested in the detailed account of historical development of theories of brain functioning and organization may be referred to Morgan (59), and Boring (11).

The birth of physiological psychology has its origin in a parallelistic view of mind and body relation put forward in the 17th century. The problem of functional localization in the brain became prominent in the 19th century.

Originally the theories lacked experimental method and were based on speculation. Right from the start there have been two opposing schools. On

the one hand the theory originated by Gall (25, as cited in Head, 32, pp. 1-12) argues for an extreme form of localization of both primary sensory functions and the "higher mental functions". On the other hand the theory originated by Flourens (23, as cited in Boring, 11, pp. 62-67) upholds the view that the primary functions are localized in the brain but that the "higher mental functions" are not dependent on any particular part of the cerebrum.

As a reaction to the early confusion and controversy that had been brought about by untested speculation there came an era of experimental investigation in neurology and psychology. The original crude clinical method which merely tried to correlate clinically observed psychological changes with brain lesions gave way to more systematic investigations. Attempts have been made to improve techniques of localizing brain lesions and whenever possible clinical symptoms have been correlated with anatomical conditions of the brain observed at post-mortem examinations. Also objective psychological tests have been introduced to measure the actual abilities of neurological patients. With the rise of experimental physiology, anatomy, and neurology the gradually accumulating findings of these disciplines have been utilized by clinicians. Eventually with the advent of surgical operations of the brain the post-operative changes in the behaviour of patients have been carefully investigated. Owing to the fact that the human material provided by nature for study is limited and cannot easily be manipulated experimentally recourse has been had to experimental studies on animals. Of great help to the problem of localization of functions have been electrical methods of stimulation and recording. By means of such stimulation the areas of the cerebrum that mediate sensory experiences and motor functions have been mapped out.

Despite the vast amount of research on cerebral functions since the time of Gall and Flourens the main issue has not been settled. Recent notions although re-phrased in more sophisticated terms and supported by experimental findings, show that the original problem remains. Most of the exponents of theories of brain functioning and organization have endeavoured to correlate the findings of neurophysiology and psychology in the development of their theories; at least they have attempted to avoid obvious contradictions. Thus one finds a certain degree of correspondence between the theories put forward by these two disciplines. There have, in fact, been three main psychological theories which correspond to three main neurophysiological theories.

Associationism or anatomical theory put forward by Henschen (38, as cited in Weisenburg and McBride, 102, pp. 29-31), and Kleist (44, as cited in Weisenburg and McBride, 102, pp. 31-33) which argues for a strict localization of function. Nielsen (61) is the modern exponent of this theory. The evidence for this view was obtained from studies of aphasic patients and supported by findings from electrical stimulation of the cortex in conscious human patients. Henschen collected evidence to show the exact location of the specific cells "endowed by nature" with the property of receiving specific impressions. Corresponding to this theory one finds so-called molecular, switchboard or connectionistic theories of learning in neurophysiology. In addition to the fact that they consider what happens in neurons, during the process of learning, they have a common premise that learning is merely the laying down of new pathways in the nervous system, between cells in the sensory system and cells in the motor system. In other words the cortex functions rather like a telephone exchange.

Field theory or theory of equipotentiality put forward by Goldstein (28), and Lashley (47) argues that the brain functions in a unitary fashion; and that there

is no localization of "higher mental functions" in the cerebral cortex at all except for the primary sensory and motor functions. Both these writers were sensitive to the claims of the Gestalt school of psychology. Goldstein, following studies of aphasic patients, came to the conclusion that the various symptoms of aphasia are manifestations of a single functional disorder, namely the loss of the ability to grasp the abstract nature of a process. Lashley, experimenting with rats, provided considerable support for this view demonstrating the "equipotential" nature of cerebral activity in the rat, and initially he maintained that there is no difference between the nature of cerebral organization in man and in the rat.

Corresponding to the above view are molar theories in neurophysiology. In order to provide for perceptual generalizations these theories oppose the molecular view and deny that learning depends on the establishment of connections. They utilize "field" conceptions, instead. Learning consists in the formation of new or altered patterns of neural activity throughout the brain (Lashley (47)).

Regional equipotentiality or functional equivalence theory argues for a limited form of localization. In this respect this theory is intermediate between the two extreme ones presented above. Head (32) upheld this view maintaining that a "higher mental function" may depend more upon one area of the brain than another, but that almost any "higher function" involves many aspects of the activity of the brain as a whole. Following studies of aphasic patients, he stated that underlying each clinical variety of aphasia is some affection of symbolic formulation and expression. Lashley (49) later modified his own earlier theory and expounded the view, similar to Head's, of regional "equipotentiality" or "functional equivalence of parts within specific areas".

The molar theories of connectionism in neurophysiology correspond more or less to the above view. These theories are intermediate between the two extreme ones, i.e. molecular and molar. Like the molar, they are mainly concerned with the construction of general models describing neural activity in the learning process, however, they are connectionistic inasmuch as they assume that the changes occurring in learning are to be found in particular neurons and synapses and that new pathways are formed. Learning takes place in particular areas in the cortex (Pavlov (66)). Hebb talks of "limited equipotentiality in the sensory projection areas" and of "limited equipotentiality between cells that are in functional parallel in physiological systems" (36, p. 40).

EVIDENCE FOR AND AGAINST EXISTING THEORIES OF LOCALIZATION OF FUNCTIONS

For the purpose of this paper psychological test findings will be mainly considered under this heading. The literature on this subject is vast and although most of the major papers have been consulted, only some of them can be mentioned here.

Each of the above mentioned theories has been backed in some degree by experimental facts and there is no disagreement with respect to primary sensory and motor functions; it is generally accepted that these are pretty well localized in very limited parts of the brain. The disagreement centres entirely around the "higher mental functions".

The Associationism or Anatomistic Theory

As it has been pointed out, the evidence for this view came from experiments in electrical stimulation and from studies of the aphasias. Both have been

criticized. Cortical stimulation has considerable limitations. Firstly, as Penfield and Rasmussen (71, pp. 202–235) have demonstrated, it produces some effects in so-called “excitable” areas, i.e. primary sensory and motor areas, and in temporal lobes, but has no obvious effects in the cortex which lies to the pre-central gyrus, in the anterior region and in the parietal cortex posterior to the post-central gyrus. Thus the strict localization shown by these experiments was of simple sensory and motor functions only, and it is not possible to generalize from these findings to the “higher mental functions”.

Secondly, such stimuli are essentially unnatural and as Fulton (24, p. 264) pointed out, one cannot stimulate a single “spot” of the cortex without affecting other “spots”. Such stimulation is bound to involve some “simultaneous units” which generally discharge in definite temporal sequence. “One cannot excite a single muscle through cortical stimulation without affecting other—usually antagonistic—muscle groups. It usually evokes patterns of movements rather than isolated responses of individual muscles” (24, p. 274).

Thirdly, in the application of such techniques to the diseased brains of conscious human beings, all the difficulties connected with the localization of function using brain-damaged subjects (to be discussed later) arise in connection with the stimulation technique also. There is also the evidence that the stimulation of the same cortical “spot” on different occasions in the same individual, may evoke different responses. Pampiglione and Falconer (64) have reported that clinical and electrographic features varied considerably from one patient to another and even in the same patient for similar parameters of stimulation of the temporal lobe cortex.

With regard to studies of aphasic patients Weisenburg and McBride (102) pointed out that Kleist’s and Henschen’s work suffered because the examinations described were not sufficient to show important characteristics of language and behaviour.

Furthermore the lesions in the brains of their subjects were not so precisely delimited as to justify the conclusion of neat relationships between lesions and dysfunctions of speech. In their own study Weisenburg and McBride (102) demonstrated that functional deficits produced by similar lesions showed great individual variations.

Conrad (15) studying 216 subjects with aphasic symptoms, whose lesions were more satisfactorily localized, showed that damaged areas causing different types of aphasia were widely scattered over the dominant hemisphere and did not correspond to the departmental view.

More recent psychological studies provide additional evidence (to be presented later) that the anatomistic theory is untenable. An excellent paper by Lashley (48) criticizes the switchboard theory from a neurophysiological and psychological point of view, quoting a considerable amount of evidence for his criticisms. It is clear from Lindsley’s (51) account of recent developments in neurophysiology that the molecular theories of brain functioning are no longer fully acceptable. Hebb (36, p. 41) points out that no one now believes that direct connections are acquired between individual cells. The conception of gradients of neural activity, in the constantly active brain, has now been firmly accepted. “The question remaining is whether gradients and fields are the only mechanisms of selective neural action or whether they are combined with an equally important mechanism of connections and specialized conducting paths” (36, p. 41). It is pertinent to quote McV. Hunt and Cofer in connection with the anatomistic theory of brain functioning: “Psychologists concerned with learning and its underlying neural basis should not be surprised at the individual functional

variability produced by the same lesions. The performances, linguistic or non-linguistic involved in the various tests must be products of the individual's past behaviour. What a priori basis is there for assuming that a given instrumental habit or skill should have the same architectonic organization in the cortex of every individual when it is well known that different cues and different muscle systems may serve the same instrumental acts in different individuals and in the same individual at different times" (40, pp. 1011-1012).

Field Theory or the Theory of Equipotentiality

A considerable amount of evidence has been provided for this "unitary" view of brain functioning from studies of aphasic patients and of animals. From the frequent coincidence of the intellectual disturbances with language disabilities an intimate association between language functions and abstract thinking was inferred.

This inference has had a profound influence on interpretations of the nature of language functions and theories of brain organization. Goldstein and Scheerer (28) concluded, from the observations of expressive aphasics with frontal tumours, that aphasia was associated with impairment of the attitude towards the abstract and categorical. This personality change is to be found especially in cases of lesions of the frontal lobes but it may result to some extent in cases of lesions of other parts of the cortex. These authors also maintained that this change is present to a greater degree in patients with lesions on the left hemisphere than with lesions in the right hemisphere. However, severe criticism has been directed against Goldstein's work. There has been no agreement concerning what Goldstein's tests measure; his emphasis on extensive studies of individual patients, without control data, affords no statistical information and lends itself to over-generalization.

Weisenburg and McBride (102) opposed Goldstein's view because their cases showed no evidence, so far as the test findings were concerned, for assuming such fundamental, universal disturbance as Goldstein described.

More damaging evidence comes from studies which attempted to test Goldstein's hypothesis more directly. Meyers (57) found that aphasic patients perform as well as their matched controls on non-verbal tests of inductive reasoning. McFie and Piercy (53, 54) reported that results obtained from testing patients with localized cerebral lesions on Wieg's Sorting Test do not support Goldstein's view that a cerebral lesion results in impairment of abstraction irrespective of its location; neither do their results confirm the view that dysphasia is contingent upon impairment of abstraction. Brown (14) showed that no significant difference in abstracting ability, as measured by Goldstein's and Scheerer's Tests, was found to exist between dysphasic patients and normal controls. Bauer and Becka (7) demonstrated that aphasic patients did not show greater impairment of abstract ability than other brain-damage cases and many of the aphasics displayed highly abstract behaviour. The results of these studies, incomplete as they are, throw considerable doubt on the validity of Goldstein's theory; it appears that aphasia is a linguistic disturbance which does not necessarily affect all aspects of intelligence. The evidence for the "regional equipotentiality", which will be presented below, will add further weight to this criticism of Goldstein's theory. The theory of equipotentiality was developed by Lashley (47) from an extensive series of experiments on maze learning in rats. He concluded that the degree of impairment of maze learning in rats was proportional to the extent of cortical surface destruction (mass action) and unrelated to the locus of the damage (equipotentiality). His work continues to

exercise considerable influence due to the extrapolated hypothesis that the human brain also works in an unitary fashion despite the fact that Lashley himself (49) modified this view. This view, as applied to human beings is not in fact acceptable, for the following reasons:

The findings of animal extirpations are of little value in determining cerebral functions and their localization in man because of the general trends of neural development toward encephalization. Higher cortical centres, as they gradually evolve phylogenetically from a relatively undifferentiated structure of tissue (pallium), displaying more generalized functions (status of lower animals), become gradually structurally differentiated, more complex and increasingly specialized with regard to functions which they take over from lower centres. Dandy (17) has pointed out that the advent in human beings of "higher intellectual functions" including speech has resulted in a different organization of tracts in the central nervous system.

The reader interested in the principle of encephalization may be referred to Morgan's *Physiological Psychology* (59).

Osgood (63, pp. 474-494) in his survey of mental functions in learning points out that with regard to maze learning in the rat there is evidence for localization of "higher functions" within the cortex and for mass action of the entire cortex and for equipotentiality; however, these findings were derived from complex learning tasks performed by rats whose cortical structures are relatively undifferentiated. Although performances similar to maze-learning have not been systematically investigated in monkeys and human beings, using the extirpation method, the anatomical and neurophysiological evidence for the principle of encephalization strongly suggests that Lashley's model of brain functioning and organization is very unlikely to apply to human beings.

There is a considerable amount of evidence (to be discussed below) that "higher mental functions" are localized in specific regions of the cortex. Lashley himself, as pointed out above, modified his original view.

Lashley based his conclusions on tests measuring learning and retentive abilities, these, however, are only two aspects of higher mental activity which are not necessarily highly correlated with other mental abilities such as general intelligence. Spearman (93), and Thurstone and Thurstone (98) regarded retentivity as a different process from intelligence owing to low correlations obtained between retention and other intelligence tests. Lashley himself (47, p. 14) was aware of these limitations. Although several studies quoted by McFie and Piercy (54) are in agreement with their own findings supporting Lashley's notion that retention and learning are functions of the brain as a whole in human beings, Meyer (55) demonstrated that, if learning ability for paired associations is assessed using different modalities, specific impairment may be shown to result from a specific lesion.

It is relevant to say here a word about the work of Bender and Teuber (8) since they indicated directions in which the search for a theory of brain functioning in the visual sphere might proceed and these suggestions have been frequently taken at their face value as being applicable to the "unitary" theory of brain functioning.

It is sufficient to point out that all the alterations in function, reported by Bender and Teuber were observed in patients with posterior lesions (particularly occipital) performing on simple tasks involving visual perception and that the phenomena reported may not be found in cases with lesions elsewhere. To take one phenomenon, i.e. C.F.F. (critical flicker frequency), Battersby (3) and Battersby, Bender and Teuber (4) reported that, at least under their experimental

conditions, there is no alteration of the C.F.F. in patients with frontal lesions, but they confirmed findings of other studies that the C.F.F. is decreased in patients with occipital lobe lesions.

Regional Equipotentiality or Functional Equivalence

There is a vast amount of evidence for localization of "higher functions" within specific areas of the brain indicating that the brain does not function in a "unitary" fashion; however, the results are inconsistent, and often contradictory, as to what functions depend on which areas. It will be sufficient to quote only a few studies to prove this point. This particularly applies to the frontal lobes, as has been pointed out by McFie and Piercy (54). On the one hand various studies have found patterns of intellectual impairment common to cases of frontal lesions while on the other hand other studies either could not demonstrate any intellectual impairment or have found gross inconsistencies with regard to the nature of the deficits. Although there have been interesting studies reported on single cases of frontal lobe damages, these will not be given consideration here because only limited generalizations can be drawn from single cases.

Three investigations, i.e. Goldstein and Scheerer (28), Rylander (78, 79), and Halstead (30) have demonstrated loss of higher intellectual functions in cases of frontal lobe injury and the authors independently expressed the notion that the more complex and integrative a function is the more dependent is it on the integrity of the frontal lobes.

However, Penfield and Evans (70), and Hebb (33, 34, 35) found little evidence of intellectual impairment on a series of cases with various frontal extirpations. Hebb (35) reviewing published cases of frontal lobe removals, concluded that the existing interpretations of the frontal lobe functions are too broad and ill-defined to be meaningful; although he himself does not believe that the frontal lobes have no function at all; according to him, no one has proved dependence of any function on this area of the brain.

With respect to the assumption of the predominant importance of the frontal lobes for intellectual functioning, studies (5, 6, 7, 18, 83, 95, 97), using various tasks, have demonstrated that frontal lobe lesions, as compared with lesions elsewhere, are not more especially disruptive of performance. In particular, studies (5, 7, 95, 97) concerned themselves more directly with testing this assumption, and agreed that the results are incompatible with this notion.

Two further studies may be cited as examples of the inconsistency of findings with regard to the frontal lobes. Thus Milner (58) concluded that temporal lobe lesion cases did worse than a group of frontal and parietal lobe cases on complex visual tasks and that right temporal lobe cases showed greatest deficit on tests involving space perception, visual and non-visual. However, investigation (6) evaluated results in the light of recent theories of an integrative focus for the elaboration of visual tasks in the temporal lobes, showed that these findings are inconsistent with those of Milner. The scores of patients with temporo-occipital, parieto-occipital, or frontal lesions on visual and tactile discriminative learning tests did not differ significantly from each other.

Turning now to the non-frontal regions one finds more consistency in the reported findings than in those related to the frontal lobes. However, the results are far from being unequivocal. The most consistent claims are that left-sided lesions (dominant side) produce the greatest performance decrements on tasks involving language functions, and that right-sided lesions (non-dominant) result in greatest impairment on non-language performance tasks, particularly

those involving spatial and spatial-temporal relationships. This differential pattern of functioning associated with the two hemispheres has been demonstrated by the following studies: (1, 54, 102). Partial support for this pattern of impairment has come from Bauer and Becka (7), and Benton (9). On the other hand, Heilbrun (37), and Meyer and Yates (56) have confirmed previous findings in one respect, i.e. that left hemisphere lesions may be associated with language disturbances, whereas right hemisphere lesions do not result in specific decrements on non-verbal performance tasks.

Turning now to the effects of lesions in particular non-frontal lobes the findings are much more inconsistent than those that have been obtained on the lateral division.

As has been shown above, speech functions have been fairly conclusively localized in the dominant hemisphere, however, it is not clear at all what the boundaries are. The traditional view, held until the present time, associates posterior lesions (temporo-parietal) with receptive aphasia, whereas anterior lesions (fronto-temporal) have been associated with expressive aphasia. Weisenburg and McBride (102) agree with this view but have stressed that any lesion in these areas always results in expressive defects; thus they made the difference one of degree only. Head (32) on the other hand, does not attach any importance to receptive disturbances and associates his verbal aphasia with lesions of pre-central and post-central convolutions. A more popular view argues that the temporal region is the seat of auditory components of speech; a lesion in this region results in difficulty in the registration, retention, and evocation of verbal impression (Schuell (80, 81)). A great many studies have directed attention to the effects following parietal lesions. Semmes, Weinstein, Ghent and Teuber (84) quote some thirty such studies, most of which gathered their information from case histories. Those which did employ objective psychological tests lack the basic requirements of a valid investigation. The consensus of opinion indicates that space disorientation is the usual sequel of parietal lobe involvement. The disturbances vary from primary sensory and motor dysfunctions to disorders of "higher functions" resembling agnosias or apraxias, for which no primary dysfunctions can be elicited. The following alterations of function have been elicited: in the estimation of directions and distance of objects, body perception or external space, confusion of right and left or deformation of the visual co-ordinates, "apraxia" for dressing, route finding, place recognition, defects in constructional tasks. However, the evidence is not unequivocal with regard to the locus of the lesion; study (84) quotes some studies which demonstrated that these alterations can occur after lesions in the occipital, temporal, and even the frontal lobes. The above quoted study by Semmes, which is one of the best investigations in this field, shows quite clearly that the parietal lobes are most important for spatial ability. Brain-damaged cases whose lesions did not encroach upon the parietal lobe, did not differ in their performance from the controls. This study also shows, despite the widely accepted view, that spatial disorders are not specific to the visual modality alone. The patients with parietal involvement did as poorly on tests involving the tactual modality as on tests involving the visual modality.

Several investigators have attempted to distinguish the nature of spatial deficits and to relate them to left or right or both parietal lobe lesions. The most consistent findings are that failure on constructional tasks due to the lack of visual discriminative judgment ("spatial agnosia") follows predominantly right parietal lobe lesions whereas left parietal lesions result mainly in constructional apraxia (52, 54, 107).

Another example of inconsistency in localization of function is concerned with arithmetical ability; impairment of this ability has been generally attributed to posterior lesions. However, different investigators "localize" it in various areas. For instance Gerstmann (26), and McFie and Piercy (54) find the greatest impairment in cases with left parietal lesions, whereas Weisenburg and McBride (102) find cases with right parietal lesions to be inferior on arithmetical tasks. Further inconsistencies may be illustrated with regard to the temporal lobes: studies (7, 31, 42, 45) have found auditory impairment in patients having temporal lobe lesions. Meyer and Yates (56), and Meyer (55) have confirmed this finding but only with respect to the dominant temporal lobe. On the other hand Symmes's (94) results do not support the above findings at all; his auditory problems have been found to be non-discriminating between the group with absence of tissue in the temporal lobe and the group with intact temporal cortex. Study (83) has demonstrated that subjects with temporal lobe lesions are particularly prone to show tactual performance losses on various psychological tests. However, study (84) has shown that tactual performance on tests of spatial orientation is only impaired in cases with parietal lesions. Battersby, Krieger and Bender (6), and Bauer and Becka (7), on the other hand, have reported that decrements shown by brain-damaged groups on tests involving the tactual modality are not found to be systematically associated with any particular lesion locus. Meyer (56) has not elicited any obvious impairment of tactual learning in temporal lobectomy cases.

These apparent inconsistencies have to be explained before any conclusions can be drawn about the relationship between tactual abilities and cerebral localization.

Let us briefly examine some of the more recent, well controlled studies which used a large number of cases with various kinds of brain damage, subdivided them according to locus of lesion, included controls, when necessary, and analysed the results statistically, comparing each sub-group with controls and complementary brain sub-groups. The number of psychological tests used and abilities assessed in these studies varies from a large number to just one test measuring one ability. Such studies, although they are not free from certain weaknesses, provide the best evidence for or against the various notions about brain functioning. If all the brain injured sub-groups were significantly inferior to controls on various tests and there were no significant differences between the brain-damaged sub-groups, the "unitary" hypothesis of brain functioning would be supported. If, apart from the significantly lower performance of all the brain-damaged sub-groups, there were significant differences between sub-groups with differently localized lesions on some tests then the "unitary" hypothesis would be partially supported, as would the hypothesis of "functional equivalence" or "regional equipotentiality". If on the other hand, some of the brain-damaged sub-groups were significantly inferior to controls on some tests, while others were not, then the "unitary" hypothesis would have to be rejected and the hypothesis of "regional equivalence" would gain support.

No direct attack on this problem on a large scale has been attempted. However, some studies (5, 6, 83, 95, 97) have demonstrated that on various psychological tests brain-damaged cases as a group are significantly inferior to non-brain-damaged controls; other studies (7, 37, 54, 83, 95, 97) quoted above together with a study by Teuber and Mishkin (96) have shown that there are significant differences on some tests between brain-injured cases with differently localized lesions. Studies (14, 37, 83, 84, 95, 96) have also demonstrated that brain injury to certain areas does not result in a deficit on various psychological

tests and in fact as a group these cases perform as well as non-brain-damaged controls. Another study (6) however, has quite unexpectedly failed to support the hypothesis that locus of brain injury is a significant factor in performance decrement on learning tasks involving the visual and tactile modalities.

The studies quoted seem to provide the best evidence available, nevertheless they only provide equivocal results which prevent us from drawing any firm conclusions about the localization of functions.

Conditioning and Learning

Two important topics, i.e. conditioning and learning on the one hand, and personality factors on the other deserve a separate discussion. Since Pavlov's work on conditioning in dogs several investigators have concerned themselves with the problem of neurological changes following modifications in behaviour. Attempts have been made to establish the locus in the central nervous system essential for conditioning and learning to occur. Detailed discussion of the findings is not undertaken here since it has been done by Osgood (63).

Unfortunately all the attempts at localization have been performed on animals and for the reasons given above one cannot generalize from these findings to human beings. According to Osgood even the findings obtained from the well controlled experiments on animals have their limitations and do not permit us to draw firm conclusions with regard to localization.

It appears that the cortex is not essential for the formation of conditioned reactions, as subcortical centres are capable of eliciting such reactions. The evidence strongly suggests that conditioned reactions are simultaneously organized in the cortex and subcortical centres. However, the cortex does seem to have facilitatory and inhibitory effects upon subcortically organized mechanisms. Furthermore it appears that the afferent system is not essential for conditioning to occur, but no conclusions can be drawn with regard to the efferent side of association.

An experiment by Culler, cited by Osgood (63, p. 492), provides limited evidence that a learned modification can be traced electrically to a precise locus in the association area of the cortex.

With regard to simple learning tasks the evidence suggests that other regions (probably subcortical) can act vicariously for the destroyed parts of the cortex, whereas more difficult learning tasks seem to depend more on the cortical areas since they cannot be relearned following the extirpation of the areas responsible for the original learning. Mass action and equipotentiality appears to be evident for limited areas, i.e. in the visual cortex for discriminative tasks, and in the entire cortex with respect to maze learning.

There is strong evidence that learning functions are progressively more dependent on the cortex, accompanied by increasingly strict localization (equipotentiality within localized areas) as we move up the phylogenetic scale with increased encephalization. Thus the extirpation of visual cortex in the rat or the dog abolishes previously learned brightness discriminations, but such discriminations can be relearned. The monkey completely loses this function, while man is completely blind after such extirpation. The lack of extensive studies concerned with the problem of localization of learning and conditioning in primates and human subjects makes it impossible to draw any conclusions with regard to brain organization for these functions at the highest level of encephalization.

A recent study by Morrell, Roberts and Jasper (60) on the effects of lesions on conditioned electrical responses of the brain in the monkey, has demon-

strated that extensive ablations of the visual, auditory, and somatic sensory areas of the cortex do not result in impairment in the conditioning of electrical responses to visual, auditory, and tactile stimuli. However, it is interesting to note that epileptogenic lesions in these areas, experimentally produced, before the extirpation took place, cause marked impairment, in this kind of conditioning, limited to the use of the sensory system in which the epileptogenic lesion was located. A focus produced in the amygdaloid region detrimentally affects conditioned reflexes to both auditory and visual stimuli. A focus in the frontal cortex does not have any effect on conditioning. (It must be pointed out that none of these findings is based on more than two animals.)

A number of studies have concerned themselves with the investigation of motor and autonomic conditioned responses in psychiatric patients. The results of these studies have been very briefly summarized in an article by Reese, Doss, and Gantt (76). Since these studies mainly concerned themselves with eliciting quantitative and qualitative differences in conditional responses in organic as compared with non-organic patients and normal subjects, and as their ultimate aim was to use any discovered differences for differential diagnostic purposes, they do not throw any light on the problem of brain organization as related to conditioning. Reese, Doss and Gantt (76) concluded that patients with various intracranial pathologies show striking deficiencies in learning motor, autonomic, and discriminative conditional responses.

With regard to learning ability in human subjects the findings are not clear as to the brain organization underlying this function. Thus McFie and Piercy (54) have demonstrated that impairment of learning is related to the size of the lesion and not to the locus. Studies (6, 106) have supported the above finding in one respect, showing that the locus of lesion is not a crucial factor in producing learning impairment. On the other hand Meyer and Yates (56), and Meyer (55), have shown that auditory learning deficit follows dominant temporal lobectomy and that no such impairment can be found following this operation on the non-dominant side. Also, Meyer (55) has demonstrated that no obvious visual or tactual learning deficit can be elicited after temporal lobectomy on either side.

The findings of the above studies are of limited value to the problem of brain organization underlying learning functions. The solution to the problem may be forthcoming from a systematic investigation of learning ability using the auditory, visual, and kinaesthetic modalities, including a variety of learning tasks and a group of subjects with varied lesions in different parts of the brain.

Personality

Investigations of personality changes following brain lesions and surgical operations have been chiefly based on projective tests, particularly the Rorschach. Such studies are of little value to the problem of brain functioning in relation to personality factors. Practically all of them have attempted to derive a list of signs which would differentiate organics from other groups of patients and normals. There has been no effort made to establish the reliability of these signs. An implicit assumption has been made that the brain functions in a "unitary" fashion and accordingly samples of brain-damaged groups have been selected without consideration as to the nature of lesions or their localization. The most damaging criticism comes from a critical review of the literature on Rorschach by Payne (68). He concludes that there is little evidence that the Rorschach can be regarded as a reliable and valid measure of personality factors.

The only work that has attempted to measure personality changes on objective psychological tests, following different types of brain operations, is by Petrie (73, 74, 75), and Le Beau and Petrie (50). Although the results of this work have been concerned with frontal lobe operations only, nevertheless they are of great significance as they strongly suggest that different parts of the brain may be contributing differentially to the different personality factors.

The personality changes have been measured by objective tests which are related to Eysenck's dimensions of personality (Eysenck (19, 20)). It has been found that complete standard lobectomy, partial rostral lobectomy and circumscribed excision in the region of convexity (Broadman's Areas 9 and 10) are followed by decrease in "introversion" and "neuroticism". The more extensive the operation the more pronounced the changes which ensue. Thus the standard leucotomy produces the greatest decreases along these continua whereas the operations involving Brodman's Areas 9 and 10 result in least pronounced changes. Petrie has also shown that cingulectomy (Brodman's Area 24) results in a different pattern of changes. There is no evidence of increased extroversion but "psychoticism" decreases. The latter change is not evident following the three convexity operations.

Circumscribed excisions in the orbital region (Brodman's Areas 11 and 47) show changes in personality variables intermediate between those after the selective surgery of areas 9 and 10, and those after cingulectomy. All the above operations produce decrease in "neuroticism" but this decrease was least marked after cingulectomy.

Comparing four subjects with right frontal operation against four subjects operated on the left (all right handed), Petrie points out that the pattern of changes in those operated on the left side was closer to the effect of a bilateral operation than in those operated on the right. This limited finding suggests that the dominant side of the frontal lobes may be contributing more than the non-dominant to the personality changes after these operations.

It should be pointed out that Petrie's results would be more convincing if retest on normal controls had been included in her study and persistent statistical analysis of her data carried out.

Tests of Brain Damage

It is relevant to say a word about tests of brain damage in this subsection of the paper. Most of the investigators designing brain damage tests assume implicitly or explicitly some generalized effect of brain injuries ("unitary" view) without taking into consideration the nature or the locus of lesions. A number of such generalized effects have been utilized, e.g. the ability to abstract, spatial ability, learning, and retention, motor, perceptual abilities, and intellectual deterioration (mental deterioration index). All of the designers of such tests claim to discriminate organics from other non-brain damaged subjects. There are also tests which claim to localize lesions; Halstead (30), for instance, has developed a battery of tests which, he claims, differentiate organics with pre-frontal lobe lesions from other groups of brain-damaged patients and normals at a high level of confidence. He also (31) claims to have developed a new battery of tests which detects unilateral temporal lobe lesions.

It is not necessary to discuss the validity of tests of brain damage here, since this has been done by Yates (105). In his critical review he points out that one of the greatest weaknesses of tests of brain damage is the lack of a clear theory behind them, exclusive to brain damage. Apart from that most of them contain serious flaws and lack the basic requirements of a valid study. In

addition misclassifications reported are high and frequent claims of low misclassifications are negated by cross validation studies.

It appears that most investigators have been mainly concerned to develop tests which discriminate organics from non-organics at a high level of confidence, forgetting that despite for example high statistical significance of mean differences obtained misclassifications may still be too high for such tests to be sensitive enough for clinical use.

SUMMARY OF THE FINDINGS FOR OR AGAINST THE EXISTING THEORIES OF LOCALIZATION OF FUNCTIONS

It should be pointed out that although the above review of the literature is brief and incomplete, it is sufficient to demonstrate that this field of psychological investigation is full of inconsistencies and contradictions. Additional evidence for these conclusions is available in the review by Klebanoff (43), and by McFie and Piercy (54).

The evidence does not enable this survey to be concluded with a clearcut statement of definite principles with regard to brain functioning and organization. However, certain general impressions can be indicated.

(a) Neurophysiological and psychological evidence indicates that primary motor and sensory functions are fairly well localized in the brain.

(b) Psychological and neurophysiological evidence renders the strictly "departmental" view (anatomistic theory) of brain organization untenable at least with respect to "higher functions".

(c) The psychological evidence available makes the "unitary" or field theory, with respect to "higher functions", unacceptable in its existing forms; neurophysiological findings do not contradict it.

(d) The theory of "regional equipotentiality" or "functional equivalence" gains the strongest support from psychological findings; neurological facts do not contradict it.

(e) Existing evidence strongly indicates that more or less independent higher cognitive abilities or groups of abilities are selectively impaired by circumscribed lesions in different locations. As Weisenburg, Roe and McBride have pointed out "there are in all probability more or less independent groups of mental abilities. The typical normal adult does not show great differences in the development of these different abilities . . . In cases of brain disease, however, or at least in cases of localized brain disease, they may be affected unequally" (103, p. 129).

(f) The findings concerning what mental abilities are selectively impaired following which circumscribed lesions are not clear at all. Results are inconsistent and often contradictory.

(g) With regard to orectic factors the findings are very scanty; but they suggest that brain organization for these factors may be similar to that for cognitive abilities.

CRITIQUE

Concept of Brain Damage

Neurologists, neurophysiologists, neurosurgeons, and psychologists agree that the only factor of importance which determines whether or not a person is brain-damaged is the absence of tissue or the presence of pathological tissue. They also concur that this must be an irreversible process. Such definition is plausible as it decides which people are brain damaged but there are, however, several difficulties on theoretical and practical grounds.

In the majority of cases pathology of the brain can only be inferred indirectly by various techniques. In many ways it holds the status of a hypothetical construct linking aetiological factors with signs and symptoms. As the methods of ascertainment improve more patients are classified as brain damaged. Thus many cases of temporal lobe epilepsy, with infrequent losses of consciousness, were previously diagnosed as idiopathic epileptics, since all the investigations during life failed to reveal pathology. However, since the advent of the sphenoidal electroencephalograph (EEG) it has been frequently possible to demonstrate electrical abnormalities indicative of brain pathology in such patients (Pampiglione and Kerridge (65)).

Thus since there are various techniques employed by the respective disciplines their correspondence as to diagnosis is not perfect. The EEG expert will consider as brain damaged those cases which demonstrate abnormalities on EEG recordings. Neurologists using different techniques may not agree with the diagnostic implications provided by the EEG. The psychologist will consider as brain damaged cases showing deficits on his tests. It is expected that the correspondence between neurological and psychological diagnosis will itself be far from perfect. Most of the psychological tests are derived empirically, that is, an attempt is made to objectify mental disturbances which are regarded by neurologists as indicative of brain damage in general and also to define some disturbances which are regarded as localizing signs. During the course of obtaining normative data on his tests the psychologist relies on neurological diagnosis which is not perfectly valid; thus the validity of psychological tests cannot be higher than neurological validity; at best it can equal it. Furthermore the psychologist attempts to measure some of the mental disturbances out of a number of other symptoms clinically manifested which, together with the results of other tests, are considered by the neurologist in his derivation of the final diagnosis. A deficit demonstrated by the psychologist may not be regarded by the neurologist as an important factor in deriving his final diagnosis, he may attach greater importance to other signs. Thus the correlation between psychological and neurological diagnosis will be greatly reduced. Owing to these limitations the psychologist will misclassify all the cases which do not display a sufficient deficit on his tests.

Secondly, the above definition refers in its scope to people with brain pathologies only, and it does not say anything about symptoms or signs. However, in practice each discipline deals only with brain-damaged cases manifesting symptoms or signs. Thus clinical knowledge of brain-damaged cases and the test results characterizing them refer to a limited group of brain-damaged cases. Williams (104), Hill and Watterson (39) have reported that 5 to 10 per cent. and 15 per cent. of the normal population, respectively, manifest abnormalities in EEG records. Also Rothschild (77) summarizing the literature on neurological and psychiatric studies in senile patients has pointed out that elderly persons who are normal mentally may exhibit cerebral changes which are as severe as those observed in patients with senile or arteriosclerotic psychoses. It is clear that according to these findings there are brain-damaged persons who do not find their way to hospitals because they do not display any symptoms. With respect to the above definition the classical concept of brain damage is limited to hospitalized cases and apart from that each discipline has a specific concept of the brain damage limited by the nature of its investigatory techniques. It appears then that each of the various disciplines commonly argue illogically in arriving at the diagnosis of brain damage. They state that some brain-damaged persons manifest certain dysfunctions, then since a particular case shows such

dysfunctions so he must be brain damaged. Clinically, one's knowledge refers to a limited sample of cases with brain pathologies.

Thirdly, as applied clinically, the above definition has a descriptive, classificatory connotation without diagnostic implications. It lumps together indiscriminately patients with variously localized brain pathologies, of different nature, determined by various aetiological factors, resulting in a variety of neurological and psychological signs, and having different prognostic implications. The clinician is interested in *diagnosis*, which implies aetiology, nature, locus of lesion and prognosis. A mere statement that a patient has brain damage is of as much use to a neurosurgeon as the statement that an engine is faulty would be to a mechanic. Clinical experience with brain-damaged patients (all of whom display some kind of symptoms) has led the clinicians to assume implicitly or explicitly that pathology of the brain is the only important factor, directly or indirectly, in producing symptoms. This idea is no longer acceptable. The above studies (39, 77, 104) provide evidence that there are brain-damaged cases without symptoms. Crome (16) throws a great deal of doubt on the significance of certain lesions found on biopsy and which have been held to account for most of the temporal lobe epilepsy. Similar diffuse and macroscopic abnormalities occur in the temporal lobe of patients who do not show such a clinical picture.

These findings indicate that cerebral lesions are not the only factors in producing symptoms. It appears that it is cerebral pathology together with some other factor or factors which result in symptoms. Rothschild (77) emphasizes the factor of individual "mental vulnerability" which, when lowered reduces the individual's mental resistance. He suggests that unfavourable heredity, constitutional factors, personality characteristics, and environmental stress should be considered and that one should attempt to determine to what extent anatomic factors, and also to what extent these other factors are responsible for the appearance of symptoms.

The original definition implies a qualitative difference between persons with brain damage and without brain damage. But we have seen that as the techniques used in detecting pathology improve more patients are diagnosed as brain damaged. It is conceivable that further improvement in neurophysiological and electroencephalographic techniques will detect some pathology in many cases of so-called "functional" psychiatric disorders and the range of brain-damaged cases will again be increased.

It is possible that eventually one will have to accept instead of a dichotomous classification of brain-damaged versus non-brain-damaged, the alternative view of continuity based on quantitative differences. The clearcut brain injury would have to be regarded not as something *sui generis* but merely as an extreme case lying on a continuum stretching from the "perfectly healthy brain" to the other extreme of, say, "complete decortication". One would not ask whether a person is brain damaged or not, but how much he is brain damaged. Such a view has been expressed by Eysenck (21) with regard to psychiatric diagnosis and by Yates (105) with regard to brain damage.

It can be seen from the foregoing that the concept of brain damage implying pathology of the brain as the sole factor in distinguishing brain-damaged population from non-brain-damaged is meaningless from both the practical and the theoretical point of view.

Clinically it is useless because it has no diagnostic meaning and only some cases covered by its scope have been investigated. Clinical knowledge of brain damage is limited to cases with symptoms and pathologies detectable by various

techniques. One cannot generalize from the findings obtained on limited samples of brain-damaged cases to brain damage in general as implied by the above definition.

The assumption that brain pathology is the only factor of importance in producing symptoms is no longer acceptable. Other factors also appear to be operative. It is possible that brain pathology *per se* is not a relevant factor. If it is not a qualitatively differentiating factor then there is no point in attempting to measure its presence or absence alone. Instead one should aim at measuring its amount and correlate it with other factors which may contribute to the appearance of symptoms. Such a conception does not deny that if cerebral changes are severe enough they will produce disturbances; but only suggests that such disturbances cannot be satisfactorily explained by cerebral changes, alone. It appears that the tendency to over-emphasize the importance of brain pathology, as a causative and explanatory factor, has hampered progress with respect to elucidation of problems of brain functions. Although the discovery of a lesion and its locus has important bearings on prognostic and diagnostic implications, in itself it does not make progress towards explaining the appearance of symptoms or the neurological mechanisms responsible for such behaviour. Such an approach results in an incomplete, descriptive, and not fully diagnostic concept of brain damage. Psychologists, in particular, should not merely seek to discover and localize lesions or regard patients as passive carriers of a morbid anatomic process but should attempt to estimate how much a patient is brain damaged and to what extent anatomic factors and to what extent, as Rothschild points out, "broader biologic or more personal factors" are concerned in producing symptoms.

Such an approach should eventually result in better diagnostic tests providing all the information so urgently required by neurosurgeons.

ANATOMICAL, PHYSIOLOGICAL AND PATHOLOGICAL SOURCES OF ERROR

Localization of Brain Lesions

Before the presence or absence of a psychological function can be ascribed to a particular part of the brain the extent of anatomical destruction and physiological dysfunction produced by the lesion must be precisely determined. This is a difficult task particularly when no histological studies of the brain after necropsy are available. In clinical practice one seldom gets such information.

Difficulties and errors in localizing brain lesions are the main causes of the disappointing results of psychological studies. These difficulties and errors have been hinted at by authors like Dandy (17), Nielsen (61), and discussed at some length by Hebb (35). Only the most important points will be summarized here.

Pathological Lesions

The extent of the anatomical injury to the brain, *in vivo* whether atrophic, neoplastic, or traumatic cannot be precisely determined and it is practically impossible to determine the functional extent of a lesion. The disturbances caused, e.g. by a tumour, are not restricted to the region of brain tissue that it occupies; it can frequently induce symptoms in far distant parts of the brain either from contiguous cerebral oedema or from the transfer of intracranial pressure (Dandy (17)). A small lesion can disrupt the connections between two anatomical centres putting both of them out of action, resulting in gross psychological deficit. There is considerable evidence that pathologically affected

tissue can interfere physiologically with normal cerebral functioning in neighbouring tissue.

Hebb (35) points out that such deleterious effects could be accounted for in terms of chemical changes in pathological cells which result in an alteration of the pattern of cell action. One way in which the latter can affect adversely neighbouring cells, or even the rest of the brain, is through abnormal electrical activity (synchronous activity). The supporting evidence for this notion comes from surgical extirpations; it is known that a clean excision of pathological tissue can frequently result in the disappearance of symptoms which were present prior to the operation.

Degenerative processes and vaso-spastic episodes are of little value in the problem of localizing functions; apart from all the difficulties in localizing lesions, the former processes are widespread (Dandy (17)), the latter, exemplified by hypertensive encephalopathy, are typically transient.

Error in localizing brain lesions can be considerably reduced by information obtained from pneumoencephalographic and electroencephalographic data; the latter technique is the only one which provides information about the physiological state of cerebral tissue. However, such data are far from being precise. Additional reduction in error occasionally comes from post-mortem histological studies; but even these are not perfect since they examine "dead brains" and so they cannot determine the deleterious physiological effects of pathological tissue on the neighbouring tissue.

It is clear from the above that the extent of anatomical destruction or the probable effects of physiological dysfunction in cases of brain injury cannot be precisely delimited. For this reason studies of individual cases or of small groups of patients without controls do not provide us with acceptable results with regard to localization of functions.

Surgical Lesions

It has been maintained that the only accurate and safe data regarding cerebral localization of functions can be obtained from cases undergoing neurosurgical operations. It is argued that such operations are accurately defined, the amount of outright anatomical destruction delimited, destroying all tissue within its boundaries and leaving the remaining parts of the brain intact (Dandy (17)). However, there are several difficulties remaining. If a psychological function is to be ascribed to the removed part of the brain then it must be certain that the removal constitutes the only lesion. Hebb (35) points out that in cases of tumours the brain is frequently so much distorted that it is impossible to delimit anatomically the extent of excision. Also distant effects of a tumour due to oedema and pressure can be present. Furthermore even a careful removal of a small well circumscribed lesion which does not have any obvious effects on the rest of the brain, or excision of epileptogenic foci where there are no underlying macroscopic abnormalities, often result in post-operative trauma (scars, oedema) which will affect a small part of brain contiguous to the area removed; in all probability this area will subsequently be adversely affected adding somewhat to the total volume of tissue removed. In addition the extent of pathologic physiological changes caused by excision cannot be precisely determined.

The error in localizing surgical lesions is much smaller than that involved in localizing pathological lesions; this particularly applies to removals of small, well circumscribed lesions and epileptogenic foci. However, even the most careful investigations do not achieve perfection in determining the extent of

anatomical destruction or pathological dysfunction. Hence studies of individual cases or of small groups of patients operated upon without controls do not provide us with precise data with regard to localization of functions.

Different Effects Following Various Types of Lesions

Another source of inconsistencies in psychological results is due to disregard of the possible differential effect of different kinds of lesions. Psychologists frequently tend to lump together cases with various types of lesions or they use a sample of cases with one kind of brain pathology and generalize from their findings to brain damage in general. There is evidence that not only can different types of lesions in the same locus produce quite different results but also that similar lesions of the same size and in the same region can have dissimilar effects. Dandy (17) for example, points out that well circumscribed or encapsulated tumours may only push the brain aside without altering its functions; also infiltrating tumours may not destroy cerebral tracts and produce no loss of function. On the other hand another tumour in the same area may have widespread effects on the rest of the brain resulting in gross dysfunctions.

Vascular and traumatic aphasia may provide one with a different picture regarding language laterality than that contributed by neurosurgery. Nielsen, and Raney (62) have reported that a complete surgical removal of the dominant temporal lobe usually results in mild residual aphasia whereas the presence of a small vascular lesion can result in quite crippling effects on language function. Hebb (35) referring to various studies of frontal extirpation finds that there is an improvement in symptoms in many cases following removals of diseased tissue. Study (46) also refers to investigations of temporal and motor area operations which show paradoxical improvement after operations. They have found complete disappearance of all the sensory symptoms, including extinction phenomenon, after the removal of a parietal tumour. Hebb (33) stresses that the presence of diseased tissue may be more productive of symptoms than the mere absence of cerebral tissue. Also, he (35) points out that the size of a lesion or its concentration is not the only important factor in producing symptoms. A small pathological lesion may produce greater dysfunction than a large one. It is, therefore, to be expected that cases undergoing similar brain operations for diverse or even similar pathology will manifest different post-operative effects; a case undergoing clean surgical excision without detectable post-operative pathology will probably show fewer symptoms than another case operated on for the same reason in the same region but demonstrating clear post-operative pathological processes. Similarly a case operated on for tumour which had very widespread deleterious effects on the rest of the brain will most probably show greater loss of function than a case with a tumour not having such widespread effects.

The length of time a lesion has been present, particularly in the case of those traumatically and surgically produced is another factor which has to be considered. It is well known that many deficits manifested shortly after operation or trauma, disappear or diminish in time. Obviously quite different results will be obtained on samples of brain injured cases differing in this respect. Karl and Elvidge (41) have pointed out that it usually takes from eight to nine months before the eventual results of the operation can be established. Bailey (2) maintains that experience with epilepsy indicates that five years must pass before one can estimate the probable final outcome.

Battersby, Bender and Teuber (4), Teuber and Mishkin (96) point out that inconsistencies with regard to C.F.F. findings and judgment of vertical in

frontal lobe lesions, respectively, may be attributed to vast differences in the samples used with respect to the nature and duration of the lesions.

It is clear from the foregoing that in order to avoid inconsistencies psychologists should refrain from the indiscriminate selection of cases for their studies. Such factors as locus, nature of pathology, aetiology, and duration should be carefully considered.

Individual Differences in Brain Structure and Organization

Another possible source of error in the localization of functions may ensue from differences in the organization of anatomical and physiological brain structures in different individuals and from different methods of acquisition and development of psychological abilities. There is little experimental evidence on this subject and the available findings are of doubtful value due to the difficulties in localization of functions; it is difficult to say to what degree apparent individual differences may be due to faulty localization of lesions. However, tentative conclusions can be drawn from the available studies.

Brains differ from individual to individual in size and weight. Variations in anatomical structure and physiological organization within particular lobes have been reported.

Penfield and Rassmussen (71, p. 106) have demonstrated that electrical stimulation of the dominant temporal lobe *about* 7 cm. posterior to the anterior tip of the temporal lobe area produces aphasic disorders. Falconer* points out that temporal lobes vary considerably in size from individual to individual and with regard to the distance from the tip of the temporal lobe at which aphasic disturbances can be produced. Meyer and Yates (56) have demonstrated that various degrees of impairment of auditory learning can follow dominant temporal lobectomy in which the actual operation can for practical purposes be regarded as identical for each case.

Klebanoff (43), discussing brain-injured children, points out that the literature indicates that these children display more diffuse effects than brain-damaged adults. This is due to incomplete differentiation and integration of cerebral organization and psychological functioning in children. For the same reason greater compensation can take place through maturation. In adults an injury usually results in long-term after-effects.

Goodglass and Quadfasel (29) summarizing the literature on language laterality quote several studies which show that aphasia follows right-sided lesions more frequently in children than in adults. Also permanent aphasia is rarely seen in children after injuries to the left hemisphere. This suggests that cerebral dominance for speech is not fully established in children and a shift in laterality can take place easily. They also quote some evidence that all brain-damaged left-handed adults are more frequently made aphasic following unilateral lesions than right handers; also left-handed brain-damaged adults frequently have only transient language defects when the nature and the locus of the lesion would lead one to expect a more severe and prolonged disturbance. Thus the left handedness implies lack of complete development of dominance or specialization of one hemisphere for language functions. They conclude from the available evidence that the rule that aphasia results from a lesion of the dominant hemisphere as indicated by contralateral handedness does not apply to left handedness. Furthermore they cite several studies which demonstrate that there is no fixed laterality for all functions and the laterality for

* Falconer, M. A., Personal communication—July, 1956.

speech and handedness does not determine the laterality for other functions.

Apart from individual differences in anatomical and physiological brain organization there is another possibility which may account for inconsistencies in findings on localization of function.

Nielsen (61) has pointed out that different individuals use different methods of training in the acquisition of abilities and also rely relatively more on one modality than another. It is not surprising then to find two different individuals with differently localized lesions manifesting identical disturbances. Conversely it is possible that identical lesions confined to precisely the same area in two different individuals may result in entirely different after-effects. Amnesic aphasia can be given as one example. Different persons recall words using different modalities, i.e. visual, and/or auditory, and/or kinaesthetic, thus amnesic aphasia may result from parietal or temporal, or frontal lesions in different individuals. Conrad (15) has confirmed this. Similarly, Nielsen goes on to say, a child who has been taught reading using mainly the kinaesthetic modality will be still able to read, if, following a lesion in the visual cortex, he shows visual verbal agnosia; the same lesion in the same locus causing visual verbal agnosia in another child, who was taught reading using mainly the visual modality, will result in alexia.

This again adds weight to the warning that psychologists engaged in localization of function should not treat their cases indiscriminately and accept test results at their face value.

It is clear from the foregoing that investigations concerned with localization of function must attempt to delimit as far as possible the extent of anatomical destruction and physiological dysfunction before the absence or presence of a psychological function can be ascribed to a particular part of the brain. This particularly applies to positive findings, that is, when a deficit is detected. When the extent of anatomical destruction is known and negative results are obtained, i.e. no deficit is found, it is reasonably safe to conclude that a particular function does not depend on that region, at least, for that patient, Hebb (34).

In order to draw precise conclusions it is essential to know whether disturbances are due to (a) anatomical destruction restricted to the region of the brain occupied by a lesion, or (b) permanent damage in the remaining part of the brain, or (c) permanent physiological disturbances induced by pathology on neighbouring parts of the brain, or (d) transient physiological disturbances.

It has been demonstrated that the available techniques are far from being good enough to answer these questions precisely, therefore most of the results obtained are of little value and claims with regard to localization permit of no conclusions being drawn. Also the evidence that different after-effects can follow different types of lesions and that there are individual differences in anatomical, physiological, and psychological organization and functioning of the brain indicates that brain-damaged cases should not be lumped together indiscriminately and treated as a homogeneous group and that the results should not be accepted uncritically.

Difficulties in the Interpretation of Results

Even if brain pathologies could be localized precisely there would still remain difficulties in the interpretation of results. The term "localization" merely implies that a given cortical area is essential for a given function, i.e. an injury to an area results in a permanent loss of function. Penfield and Jasper (72, p. 240) point out that it is illegitimate to say that a function is represented in a cortical area, centre, or point. All we can say is that one part of the neural

connections involved in producing a function is to be found there. If there is no indication of any other anatomical destruction or physiological pathology outside the area occupied by a lesion and if on testing over varying intervals the dysfunction is present and cannot be regained by retraining, only then can one conclude that the given area is essential for the given function.

If on the other hand a dysfunction follows a well localized injury and is shown to be present over varying intervals but can be recovered through retraining then one can conclude that the given region is not essential for this function and some other area can take over the lost function (vicarious functioning of one area with respect to another). To trace the area of vicarious functioning one would require to demonstrate that an injury to another area eliminates the possibility of retraining. Lack of a given dysfunction following an injury does not necessarily mean that this function does not depend on the injured area at all. It may be that it is simultaneously organized in another area (replication of function). Right and left hemispheres display replication for many functions. Thus injury to either does not necessarily result in the loss of performance, and in order to demonstrate that a given function is replicated it must be shown that injury to both regions results in a loss of function.

A major difficulty in the interpretation of findings arises from the fact that some functions, initially lost, particularly after traumatic lesions and brain operation, show spontaneous recovery. It may be that a pathological area or the removal of an area, not essential to a certain function, will result in transient physiological disturbances in other cortical or subcortical regions so producing a temporary loss of performance. As these physiological disturbances subside in time the function will reappear. (Theory of diaschisis—Von Monakow, 100, as cited in Lashley, 49, pp. 5–6.) Thus one may draw wrong conclusions from the findings obtained soon after trauma or operation. Longitudinal study is essential and one must make sure that no retraining has occurred between the testing periods. In order to demonstrate that Lashley's principle of equipotentiality and mass action can be extended to human subjects it must be shown that dysfunctions due to lesions in various parts of an area are equivalent and independent of the locus of injury (equipotentiality), and the degree of dysfunction is correlated with the extent of injury (mass action).

The complexity of the problem requires a rigorous experimental approach in order to overcome the difficulties in interpreting results. Such work has been attempted on animals. It is not possible to experiment on human subjects and clinical material does not provide cases for crucial experiments; clinical cases must be taken as they come. Hence investigators must be very cautious in drawing conclusions. Most of the investigations cited fail to consider these difficulties and arrive at conclusions which are not warranted by the data.

FLAWS IN PSYCHOLOGICAL INVESTIGATIONS

We have seen that the lack of attention to localization and nature of lesions, individual differences, nature of dependence of functions on cortical and subcortical regions can result in considerable inconsistencies in findings with regard to the localization of functions. A critical evaluation of studies with respect to these factors renders most of the conclusions invalid. In addition there are serious flaws inherent in most psychological studies.

Controls

One of the most serious weaknesses in psychological investigations is the lack or misuse of control data. The pre-morbid level of psychological abilities

of brain-damaged patients is very rarely known, hence it becomes very difficult to demonstrate a deficit on a test as a result of brain injury. Test norms on comparable groups of normal, functional, and brain damaged subjects are essential. Even if the pre-morbid level is known one must obtain test retest data from various comparable groups before any possible deficit can be estimated. A real danger lies in using unstandardized tests and in assuming a "norm" for the normal population. Pearson and Alpers (69), Hebb (35), Shapiro and Nelson (91), have demonstrated that such assumptions can lead to erroneous conclusions.

A number of investigators, using or obtaining control data, disregard such important factors as age, sex, and intelligence. One *must* equate samples for these factors. Gilbert (27), and Wechsler (101), have demonstrated that mental abilities decline with age. With respect to intelligence Boyd (12) has shown that it is an important factor in performance on a test of brain damage. Shapiro and Beech* report that their conclusions about the performance of certain clinical groups on a single perceptual task have been radically changed when allowance has been made for initial differences in intelligence. Also they have been able to demonstrate that intelligent brain-damaged patients are able to compensate for perceptual disabilities on a psychomotor task. Studies concerned with localization of functions frequently come to conclusions which are not warranted by the data owing to the lack of control groups. In order to provide conclusive evidence that a given dysfunction is due to a given brain pathology it must be demonstrated that lesions in other areas do not result in such a deficit. Hence samples of patients with variously localized lesions, equated for all the relevant factors, must be included.

Nature of Deficit

Nielsen (61) maintains that cerebral activity functions at least at three levels of integration. Selective injury to respective levels can result in quite different dysfunctions. The first level is that of primary perception and with regard to vision and audition it is pretty well localized. Tactual sensibility and motor functions present much greater difficulty. The next physiological level is that of formulation of engrams for recognition. These functions have not been as precisely localized as the functions of the primary level of integration. At the highest level—level of elaboration—there is practically no localization known as yet. At a lower level these functions are concerned with the significance of stimuli, and at a higher level with the significance of stimuli within the context of stimuli in which they appear. Psychological tests are not pure measures of abilities, and apart from involving the three levels of integration, they contain both general and various specific abilities. A performance test may involve motor skills, spatial ability, visual discrimination, and other complex abilities difficult to define. For this reason failure on such a test may result from the selective impairment of any one of these abilities following injuries in various regions of the brain and at different levels of integration. Thus, if the nature of a deficit is not more closely defined by a further careful investigation, the only valid conclusion is that the deficit is a complex one causing impairment on this particular task. Teuber and Weinstein (97) rightly point out that localization of functions cannot be taken for analysis of functions involved in a test.

The brain-damaged patients' peripheral or primary motor and sensory defects, and any behavioural aspects which might affect the performance must

* Shapiro, M. B., and Beech, H. R., Personal communication—July, 1956.

be taken into account. It is just as illegitimate to give a patient a test of intelligence which is affected by his "peripheral" defects and conclude from low results that his intelligence has declined as it would be to give a blind subject a visual test of intelligence and conclude that he is mentally defective from his low score. In order to demonstrate that a particular ability is impaired as a direct result of an injury it must be shown that it is not indirectly caused by an impairment of another ability with which it is correlated. Most psychological studies are inadequate in these respects.

Meyer and Yates (56) have demonstrated that the general intelligence of temporal lobectomy cases is relatively unimpaired after operation, if assessed on tests not involving the patient's specific deficits (especially hemianopia and aphasia). Shapiro (87, 88, 89, 90), Meyer (56), and Semmes, Weinstein, Ghent and Teuber (84) have shown that wrong conclusions can be drawn about the nature of various deficits demonstrated by brain-damaged cases on various tests, unless these deficits are examined under conditions of careful experimental control.

Empirical Approach

The basic flaw in most of the studies lies in their theoretical approach. The most commonly practised approach is the empirical one. This consists in the administration of a battery of standardized or unstandardized tests, assumed to be pure measures of various psychological abilities, without any clearcut formulations and hypothesis as to possible outcome of test results. This type of approach is bound to be fruitless even if methodological flaws are eliminated. The lack of any hypothesis prevents one from making deductions, selecting appropriate tests and setting up adequate experimental situations to test these deductions. The empirical approach is insensitive in the detection of deficits and in aiding an understanding of their nature.

Bailey (2) has pointed out that he does not believe that any part of the brain can be removed without producing a defect of some sort. If a battery of tests does not demonstrate a deficit it means that the psychologist did not know what question to ask. He goes on to say that standardized tests used in studies of some brain regions may be successful but it does not follow that success will be achieved in studying other regions with them.

At the present state of knowledge of brain organization and functioning it is practically impossible to postulate a clearcut hypothesis as to the possible outcome of a specific injury on psychological functioning. Nevertheless the neurophysiological and psychological literature is abundant in information from which certain leads or expectations can be derived. It is essential that the relevant literature be consulted before an investigation is undertaken.

ARE PSYCHOLOGISTS JUSTIFIED IN REFERRING BEHAVIOURAL DATA TO THE CENTRAL NERVOUS SYSTEM?

So far this paper has been concerned to make destructive criticisms. Before any attempt is made to suggest what seems the most fruitful approach let us critically examine whether psychology as a science of behaviour in its broadest sense is justified in making any reference to the central nervous system. Skinner (92) holds the view that the very idea of neurological correlates of behaviour implies that there are two independent subjects to be studied, i.e. the nervous system and behaviour, and each discipline must have its own techniques, methods, and different kind of data. He maintains that at the present level of development and knowledge both sciences, i.e. neurology and

psychology, can proceed independently. The procedures which attempt to relate behavioural facts to their neural correlates, instead of studying the facts of behaviour as such, prevent the development of a science of behaviour. This tendency to look to the nervous system for an explanation of behaviour is a common practice among clinical psychologists. Skinner rightly points out that: "The discovery of a cerebral lesion as 'the neural correlate' of, let us say, aphasia is doubtless an important step in understanding of the condition of a patient. But the success in this instance of finding 'what is wrong' with behaviour by looking into the nervous system depends largely upon the negative nature of the datum. The absence (and in many cases the derangement) of a function is much more easily described than the function itself. 'He speaks' is admittedly an inadequate description of verbal behaviour which demands greater amplification. 'He cannot speak' is a fairly complete description of the opposite case, so long as the unanalysed notion of speaking is accepted. The significance of this difference for the present argument may be pointed out by comparing the correlation of aphasia and a lesion with correlation of normal speech and the neural processes involved in it" (92, p. 424).

It seems that Skinner is right in that clinical psychologists should not stop at mere correlations but should proceed to demonstrate their significance. As students of human behaviour their duty is to describe the nature of deficits, derive laws governing them, and attempt to control them. Such an approach will not only sharpen their diagnostic and prognostic abilities but will also enable them to develop a psychological theory of brain functioning and organization. Careful study of correlations between behavioural and neurological facts are essential and basic to the development of such a theory. The nature of a clinical psychologist's work demands that he should not remain oblivious of neurological facts and theory. He can no more contradict the known facts of neurology, than the neurologist can contradict the facts known about human behaviour.

"To understand something of the mechanism and the requirements of the nervous system's role in behaviour will provide anchoring points from which behavioural and psychophysical studies may proceed with greater assurance and with greater benefits to psychology and neurophysiology alike" (Lindsley, 51, p. 344).

Skinner's argument that the science of behaviour can make little use of the practice of the clinician, except in so far as it is descriptive, is not acceptable. Carefully described abnormal behaviour in brain-damaged patients can throw light on the nature of normal functioning in the intact organism. Bender and Teuber (8) have pointed out that the performance of the brain-damaged organism may reveal normal functions which are less apparent in the intact organism. Shapiro's theory of brain damage (87, 88, 89, 90) developed from a study of a perceptual anomaly in brain-damaged subjects throws light on perceptual behaviour in normal subjects.

Eysenck (22) has shown that studies in the field of brain damage have made contact with his theory of extraversion/introversion which was developed on a non-brain-damaged population.

Suggestion for a More Fruitful Approach

The most fruitful psychological approach to brain damage would obviously be one which would lead to the development of an acceptable theory of brain functioning and organization to account for behaviour in general and for the

abnormal behaviour of the brain-damaged. As a result of such a theory one could construct sensitive tests of brain damage which would not only differentiate brain-damaged patients from non-brain-damaged but also indicate the main diagnostic implications. To achieve this a complete reorientation must take place in this field. It has been shown that a vast number of isolated, disconnected studies have been reported, results of which do not permit of a clear orientation in the field.

Two ways seem to be indicated by which a better orientation could be achieved. These are not mutually exclusive but complementary. The first one suggests itself from the field of intelligence and personality. There has been much controversy in both spheres between "specificity" and "generality". Vernon (99) has found that the evidence supports a hierarchical view of intelligence. Eysenck (19, 20) has demonstrated that personality factors are likewise organized in a hierarchical way. We have seen that the same controversy persists with respect to the brain organization and functioning.

It is plausible to postulate, from the evidence of cognitive and orectic studies and from the available data in the field of localization of functions that the brain is also arranged in a hierarchical way.

It has been pointed out that the evidence from studies on brain damage strongly suggests that more or less independent cognitive and orectic factors are selectively localized. With regard to findings on existing "unitary" theories the evidence is negative.

Thus the first proper question to be answered is to what extent the brain functioning is "unitary", and to what extent "departmental". This could be achieved by a study on a large scale which would include a representative sample of brain-damaged patients and a sample of the normal population. The total brain-damaged sample would have to be subdivided according to the locus of lesion (anterior, central, posterior), by lobe injured (frontal, parietal, temporal, occipital), by involvement of one or both hemispheres (unilateral, bilateral), and by laterality of lesion (right, left). A large battery of tests should be selected including those tests which are known to be measures of well-defined orectic and cognitive factors. The results should be submitted for statistical analysis; the performance of each of these subgroups should be analysed in relation to those of the controls, and of a complementary subgroup consisting, in each case, of the remainder of the brain-damaged group. Such a study should provide a clearcut answer to the problem of "generality" versus "specificity".

The next step would be to study extensively and intensively each significant association between factors and loci of injury, controlling for all the relevant variables, with the ultimate aim of clarifying the significance of each association, including the neurological mechanisms responsible for the positive and negative properties of functions.

Similar studies should be conducted on children of various age levels with the purpose of defining the nature of developmental aspects of brain organization and functioning.

Eysenck (22) has shown in the sphere of personality how an intensive study of statistically derived personality factors can arrive at an explanatory theory to account for their existence.

There have been several studies reported (6, 7, 83, 84, 97) which, in order to test particular hypotheses, have made the first step of the approach advocated. However, they have been performed on a small scale and have never progressed beyond obtaining significant relations between brain injuries and dysfunctions.

The other approach has been put forward by Payne (67), and Shapiro (86) and applied by them to brain-damaged and psychiatric patients. They have advocated and conducted systematic, objective investigations of a single abnormality or dysfunction on a single case with the main purpose of defining its nature, which in turn is intended to lead to an improved understanding of the patient's abnormality as a whole. Using this approach Shapiro (87, 88, 89, 90) developed a theory which is exclusive to brain damage and which maintains that brain damage results in the development of states of exaggerated inhibition. Shapiro's theory to account for the effects of brain damage is the only one so far acceptable. However, it has been developed and tested in one modality only, and his test of brain damage, constructed on the basis of the theory, shows about 40 per cent. of misclassification between the brain-damaged and non-brain-damaged groups. The reason for such a high misclassification lies, perhaps, in the assumption that the brain functions exclusively in a "unitary" fashion. Additional tests in the auditory and kinaesthetic modalities will no doubt reduce the misclassification. However, it is doubtful if the misclassification will be completely eliminated by this addition if only for the fact that no modality has been shown to be "localized" in the frontal lobes. It is possible that the frequently reported cognitive deficits in cases of frontal lobe lesions are not directly produced by any pathology but are the indirect results of frequent personality changes, e.g. a lack of motivation, an inability to plan and to maintain goal (Karl and Elvidge (41)). Hence unless other relevant factors are incorporated in this theory and their relationships clarified, this test will never achieve diagnostic status in the strict sense.

It would appear that the clinical psychologist's contribution to the problems of the neurosurgeon and the psychiatrist is of little value owing to the lack of proper diagnostic tools. However, it is the writer's opinion that the psychologist should not emphasize the diagnostic aspect of his work, even if he is supplied with relatively adequate classificatory tests of brain damage. His main purpose should be to detect abnormalities, formulate explanations to account for them, and to test deductions ensuing from his hypothesis. Such a method of investigation should enable him to describe the abnormality precisely, to demonstrate under what conditions it is modified and eventually, with all the information available, he should attempt to train the patient with the view of amelioration or even with the view of complete cure of the abnormality. The widely held view that consequences of brain injuries are irreversible has prevented progress in this field. Schuell (80, 82), and Birch and Lee (10) have demonstrated that using an acceptable theory to account for an abnormality manifested by brain-damaged patients one can design successful treatment. They have, for example, successfully treated aphasic patients.

Some of the psychologist's findings will provide a basis for further fundamental research. Some of his findings will be incorporated in the development of the theory of brain organization and functioning. However, no matter how far fundamental research progresses in developing a theory of brain organization and functioning, the clinical approach to individual cases will have to be maintained due to the subtle individual differences which require a careful and precise investigation of each case for the purposes of treatment and disposal. In all types of work the clinical psychologist must bear in mind that just as much as in fundamental research, he will be useful precisely to the extent that he can bring his training in scientific method and his knowledge of psychological principles to bear on the problems presented to him in each individual case.

SUMMARY

(a) A historical development of theories of brain organization and functioning has been briefly discussed.

(b) Each theory has been evaluated in the light of neurophysiological data and some of the findings of psychological investigations bearing on the problem of localization of functions.

(c) Some reasons for the inconsistencies on psychological findings, and for the failure to develop an acceptable theory, have been enumerated.

(d) The concept of brain damage and the problem of whether the psychologist is justified in relating behavioural data to the C.N.S. have been discussed.

(e) Some suggestions towards a more fruitful approach have been presented.

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THE USE OF SOME NEW PHARMACOLOGICAL AGENTS IN PSYCHIATRY*

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THE last five years have seen the beginning of a remarkable new development in the pharmacological approach to psychiatry. Pharmacological treatment for mental disorder is not, of course, new; drugs like hellebore, opium, camphor, bromides, paraldehyde and the barbiturates cover a large part of the history of psychiatric therapy. Since 1951 a number of new drugs with the generic title of tranquillizers have been introduced, of which the two best known are chlorpromazine and reserpine. They have the characteristic effect of producing sedation without producing sleep.

This paper deals mainly with the clinical aspects of the use of chlorpromazine, reserpine, and a new phenothiazine derivative, Pacatal or Lacumin, in psychiatry. We would first like to refer briefly to some of the recent discoveries that have been made concerning the mode of action.

There is evidence that both drugs act on the mesencephalic and diencephalic parts of the brain, especially the hypothalamus and the mesencephalic alerting system of Moruzzi and Magoun (1949) and Jasper (1949). Hypothalamic function may be suppressed or reduced; for example, autonomic activity, the rage centres, metabolic functions, and temperature control may all be inhibited (Courvoisier *et al.*, 1953). Through the hypothalamus both chlorpromazine and reserpine influence endocrine function, causing lactation and menstrual disturbances. The hormonal imbalance which produces this action is thought to be due to a suppressor effect in these areas (Sulman and Winnick, 1956). Though both cause a fall in blood pressure through their action on the hypothalamic autonomic centres, chlorpromazine increases the heart rate while reserpine slows it, perhaps due to differences in their peripheral actions, since chlorpromazine is known to have an atropine-like activity (Bein *et al.*, 1953; Burn, J. H., 1954).

The mesencephalic alerting system influences general cerebral function and behaviour; it acts as a vast relay and distribution centre on the afferent side—stimulation produces wakefulness and alertness, while depression induces sleep. Both states are accompanied by characteristic EEG activity. Stimulation of part of this system produces Parkinsonism as has been shown by Ward, McCulloch and Magoun (1948). In small doses reserpine stimulates this alerting system and chlorpromazine sedates it, but in larger doses chlorpromazine is said to stimulate it and it is notable that both drugs in large doses can produce a Parkinsonian state. The importance of these areas, and also of the

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related limbic, hippocampal, and temporal areas, in disturbances of behaviour and emotion and also in the regulation of the level of awareness and consciousness have been stressed in recent years (Ingram, 1952; Pond, 1954). The part they must play in the regulation of the disturbances of function met with in psychiatry has been emphasized by Gellhorn (1953) among others, and it would seem that drugs which particularly influence these parts of the brain might be expected to affect disturbed behaviour profoundly.

Professor Elkes (1956) has pointed out that the overall effect is a complex result of inhibition and stimulation in these extremely important homoeostatic centres. From the chemical point of view it has been suggested that a possible clue to the action of these drugs is through their effect on the substance serotonin or 5-hydroxy-tryptamine (Gaddum, 1953). Serotonin is a neurohumoral agent present in brain tissue and plain muscle. Pletscher and others (1956) have shown that reserpine leads to a diminution of serotonin in the brain, and Wooley and Shaw (1954) have suggested that excessive serotonin may result in abnormal mental states. They have also suggested that reserpine may block receptors normally sensitive to serotonin. Chlorpromazine has also been shown to block some of the effects of serotonin, though its action is not as clear (Gyermek, 1955). Serotonin is linked with the action of LSD-25 and mescaline and is inhibited by these substances. These findings are incomplete, and though there is as yet no satisfactory theory to account for all the activities of these drugs, a new and exciting field for speculation and experiment has been opened. It has been suggested that reserpine, serotonin, the hallucinating drugs lysergic acid and adrenochrome, postulated as a breakdown product of adrenaline in the body and an aetiological agent in schizophrenia, all have a part of their structure in common (Himwich, 1955). The relationship of these substances to each other may be similar to that between vitamins and anti-vitamins. Further advances may throw light on aetiological factors in mental disorder.

CHLORPROMAZINE

Most of our experience has been with chlorpromazine and we would like to discuss this first.

Chlorpromazine is closely related to various other pharmacologically active phenothiazine compounds such as phenergan and lysivane. It shows anti-adrenaline, anti-acetylcholine, and anti-histamine activity, as well as a strong local anaesthetic action, and potentiates barbiturates and other anaesthetics and sedatives. As already suggested, multiple actions of all these drugs must be borne in mind when interpreting their clinical effects (Burn, 1954). Under the influence of chlorpromazine patients may appear dreamy, calm, listless, indifferent, and apathetic, but the overall effect is unlike that produced by barbiturates, which render the patient drowsy. Contact can readily be made with subjects under its influence, though patients seem indifferent to the environment and show lessened anxiety. Early this year we reviewed the literature since 1952, from which the general uses and limitations of the drug were apparent (Leiberman and Vaughan, 1956). The largest number of papers dealt with acute or chronic psychotic states, which are in fact the main indications for its use. Other indications are confusional states of various aetiology, withdrawal symptoms in drug addiction, acute alcoholism and antabuse intoxication, and the restlessness and overactivity (particularly at night) of senile demented patients. Some dyskinetic neurological disorders such as Huntington's chorea have been benefited as regards the movements and it has also been

used in the overactive behaviour disorders of children. In fact psychomotor overactivity would appear to be the group of symptoms most influenced, rather than any particular nosological category. It is not particularly successful in neurotic states or depressive conditions. It can be combined with other physical treatments, such as insulin coma or E.C.T.

Last year we reported on a series of patients treated with chlorpromazine and since then our experience has largely confirmed our earlier findings (Vaughan, Leiberman and Cook, 1955). Cases of relapsing depression which respond for a short time to E.C.T. showed the best response in the affective group. Chlorpromazine following E.C.T. seems to help these people to maintain a steady state.

About half the patients with mania responded quickly, and more might have done so had our doses been higher. It is probably best combined with E.C.T. in this disorder; the E.C.T. to get the condition under control and the chlorpromazine to prevent any relapse. Our most impressive results were among chronic schizophrenic patients; 28 of 103 were much improved, and an analysis of their symptoms showed that improvement was largely in terms of their overactivity, aggression and excitement. Improved patients do not appear bemused or befuddled as is the case with other sedative drugs, but become more interested and active, eat better, and start to communicate with others. A follow up some 12 months later of 25 of these patients gave the results as shown in Table I. With the aid of chlorpromazine many chronic patients are being

TABLE I
One Year Follow Up of 25 Chronic Psychotic Patients Who Had Originally Improved on Chlorpromazine

Number of patients	..	10			15
Duration in hospital	..	7-44 years			1-6 years
Discharged	..	1	Discharged	..	4
On trial	..	2	Symptom-free in hospital	..	2
Much improved: minimal symptoms	..	2	Much improved: minimal symptoms	..	5
Improved: symptoms present	..	5	Improved: symptoms present	..	4

helped to go home on trial or are being discharged, and many more maintained in a steady and active state within the hospital environment, with a beneficial effect on chronic disturbed wards. Their relatives find a new interest in their welfare and this further helps in their social recovery. When the most disturbed members of the community become quiet and co-operative, other patients also become more settled and socially improved. A number of reports (Gatski, 1955; Bair and Herold, 1955) have now appeared on its use in overactive and aggressive children, and though one may hesitate in using a potentially toxic drug on children, there may be justification where it might make the difference between having to remove the child from his home, or giving the child sufficient respite from overactivity, aggression and anxiety to give some rest to the parents to allow the family situation to settle. It has been used by one of us (G.F.V.) with good results in 23 such children, and in five cases of severe habit spasm, where it was very successful in three. It may be useful in the restless psychomotor overactivity complicating brain damage. For example, a girl of 6½ years with brain damage due to birth injury showed restlessness,

aggression, screaming attacks and severe insomnia. Barbiturates and the amphetamine group of drugs made her more restless. On chlorpromazine 120 mg. per day, she sleeps at night, is constructive in her play, and has settled at school where she is now beginning to progress.

Dosage up to 2,000 mg. a day can be tolerated by adults, though at these levels Parkinsonian symptoms may develop, and there is probably no advantage in exceeding 800 or 1,000 mg. daily. For rapid action the intramuscular or intravenous routes should be used. The 2.5 per cent. solution should be diluted about 10 times with saline before being injected intravenously. Deep intramuscular injection is well tolerated though it may occasionally cause painful areas of induration. If some improvement is not shown in one month it should be discontinued, though once improvement has started it may continue for a considerable period.

Jaundice and agranulocytosis are the complications most to be feared. Although we have experienced a number of minor complications, we have not encountered serious trouble apart from one case of agranulocytosis which recovered, and a few cases of jaundice when we first started using the drug. It is often possible to recommence the administration of chlorpromazine with no untoward results after it has been stopped for some complication, though agranulocytosis should be an absolute bar to its use again.

RESERPINE

Rauwolfia Serpentina is a common plant in some provinces of India and has been used for centuries in the treatment of insanity and anxiety by the Ayurvedic system of medicine. It was introduced into scientific medicine as a result of the investigations of a group of Indian pharmacologists and physicians from 1931 onwards (Weber, 1954). The alkaloids Reserpine and Rescinnamine have the most potent sedative and tranquillizing effects. The other alkaloids, of which there are a number, have not been fully investigated from the psychiatric point of view.

In 1953 Hakim reported on the use of *Rauwolfia* in mentally disturbed patients, and since then there have been many reports, mostly from America, on the use of reserpine in psychiatric practice. The general indications for treatment follow closely those for chlorpromazine. Thus it is not particularly successful in psychoneurotic states and the more severe depressions (Davies and Shepherd, 1955; Folkson and May, 1955). Indeed its use is attended with the risk of severe psychotic depression developing, and this has been noted with the comparatively small doses employed in treating hypertension and the larger doses used in psychiatric practice (Muller *et al.*, 1955; Smirk, 1955; Schroeder and Perry, 1955). This depression may be treated with analeptic drugs (Ferguson, 1955) or with E.C.T. (Moore, 1956), and treatment with reserpine can then be continued. The most satisfactory results have been obtained in conditions of psychomotor overactivity, restlessness, aggressivity, violence, impulsiveness, etc., associated with acute or chronic psychoses. Very impressive results have been reported by a number of authors in recent and chronic schizophrenia, and typical of these are the papers by Kline and Stanley (1955), Hollister *et al.*, (1955) and Moore (1956). There are reports of its success in treating mania (Kinross-Wright, 1955), senile psychoses (Sainz, 1955), confusional states (Avol and Vogel, 1955), drug addiction withdrawal symptoms (Carey, 1955), neurological disturbances, e.g. Huntington's chorea (Bleuler and Stoll, 1955), and the behaviour disturbances of children (Zimmerman and Burgemeister, 1955); a list which closely parallels that for

chlorpromazine. There is no unanimity about dosage, except that it is usually necessary to exceed 3 mg. per day and typical of most schemes are those for example of Kline and Stanley (1955) or Moore (1956). They give 1 mg. t.d.s. by mouth with 5 or 10 mg. per day intramuscularly, and, depending on the response, continue this high dosage for two weeks, then reducing it over a month to seven weeks. At the end of that time the patient is receiving orally about 3 mg. per day. Like chlorpromazine, Rauwolfia should be continued for at least a month before deciding that no therapeutic response is possible, and it may be necessary to continue for months before the maximum effect is seen. It can be administered intravenously for rapid action. Patients treated show a series of symptoms and signs such as falling blood pressure and bradycardia, slowing of movements, loss of spontaneous movement, torpor and drowsiness, and marked increase in appetite, but they retain a clear sensorium and can be readily roused. In association with this a peculiar type of restlessness may develop, termed the turbulent phase by Barsa and Kline (1956) which should not be regarded as an indication for stopping treatment. Barsa and Kline (*loc. cit.*) have described three stages, sedation, turbulence, and integration, in patients who respond to the drug, though it is our experience that this sequence is not necessarily followed in every case and may not occur at all. Common complications are fatigue, lethargy, postural syncope, dizziness, coldness and shivering, a severe Parkinsonian syndrome, diarrhoea, oedema of the legs and skin rashes. Nasal stuffiness is common.

We would like to present briefly our results in a controlled study of 43 chronic psychotic patients who were of interest as they were of extremely poor prognosis.

The study was undertaken at Bexley Hospital in conjunction with Dr. F. G. Tait. The first group consisted of 20 female patients of mean age 49 years and mean duration of stay in hospital 21 years. The mean duration of their illness was 22.5 years and they all presented the various symptoms of deteriorated schizophrenia. During the experimental period the ward routine was continued as far as possible unchanged. Ten patients were given reserpine up to 12 mg. per day by mouth, and 10 an equivalent number of control (inert) tablets. Although the observers (ward staff and doctor) did not at first know which were active, the matter was not long in doubt owing to the occurrence of side effects. Over 1 month 6 of the group on reserpine showed improvement compared with 1 of the control group. They were quieter, less restless, less aggressive, with increased appetite, and were sleeping better, but in no case was the structure of the psychosis altered. These figures are significant at less than the 5 per cent. level ($\chi^2=5.4$, $n=1$). The main complications were 4 cases of Parkinsonism and 2 cases of diarrhoea. Although at the time the degree of improvement was not considered marked, the reappearance of disturbing symptoms on discontinuation of the drugs emphasized what improvement there was and led to requests for continued administration. The second group consisted of 30 female schizophrenic patients of equally hopeless prognosis, mean age 53 years, duration of illness 22 years, and 21 years in hospital. This second ward was the centre of an active rehabilitation scheme vigorously pursued by Dr. F. G. Tait. A double blind scheme of administration was adopted. The drugs were administered by mouth, and a maximum of 9 mg. of reserpine per day was given, the average being 6 mg. with a range of 3 mg. to 9 mg. For various reasons, seven patients dropped out of the experiment before it was completed, and assessment has been made of 23. Assessment was qualitative and made on the basis of social behaviour and a number of special symptoms

by the doctor in charge and the ward sisters. Blood pressures were recorded weekly. In this second group 17 patients improved on reserpine compared with 16 on inert tablets. On reserpine, however, 7 showed a marked degree of improvement compared with only 1 on the inert preparation. Improvement showed itself in reduction of tension, aggression and noise, increased activity in all the ward routines and special procedures, increased appetite and weight and improvement in sleeping as described previously. Special symptoms, e.g. hallucinations, thought disorders, delusions, were not completely lost in any case, though the force of hallucinations seemed to diminish perceptibly in several. There was usually a fall of blood pressure of 20 to 30 points systolic and 5 to 15 diastolic, but there were considerable fluctuations, the pressure tending to return to the normal value for the patient when the drug was continued. Generally speaking the results in this ward were better than in the first, perhaps due to the longer period of administration, combined with the opportunity of greater activity. The improvement could not be attributed solely to the non-specific measures employed since the most improved patients relapsed when given inactive tablets. Improvement took place almost imperceptibly on active tablets, and deterioration was similarly slow on cessation, a patient taking some 4 to 6 weeks to revert to her previous level. The usual complications of Parkinsonism, diarrhoea, lethargy, etc., were observed and responded to changes in dosage or other measures, and several patients showed the peculiar restless overactivity described by Barsa and Kline (1956), and commented on by Moore (1956), and which is regarded by them as in some way conducive to therapeutic success, but makes the patients difficult nursing problems. It should not be regarded as an indication for reduction of dosage. We would like to emphasize that treatment with reserpine should be prolonged and as high a dose administered as can be tolerated.

In chronic schizophrenic patients such as we have described, it is not likely that the improvement will be spectacular with any form of therapy. Hoch (1956) quoting American experience gives 5 per cent. as the fraction of chronic schizophrenics who will improve enough on reserpine to leave hospital, though as Hollister *et al.* (1955) have shown, the degree of chronicity is important. In their series only 5 of 21 patients with illnesses of over 13 years duration showed improvement, whereas 12 of 14 with illnesses up to 2 years did so. Although these points seem obvious, and are well known, they should be borne in mind when comparing or assessing results. Age, degree of chronicity and the other factors affecting prognosis must be taken into account.

In comparing chlorpromazine and reserpine, it is our experience that chlorpromazine is more effective, acts more quickly and is less unpleasant for the patient, while reserpine acts more slowly, over a longer period and while the number of unpleasant side effects is much greater than for chlorpromazine, there are none of the serious complications of chlorpromazine such as jaundice or agranulocytosis. Hoch (1956) thinks they both act on the same symptoms through the same mechanism. Watt (1956) thought reserpine without effect on chronic psychotic patients, while chlorpromazine was active. Kovitz *et al.* (1956) in a careful study thought reserpine and chlorpromazine of equal value with differences of action as described above. Some authors, e.g. Barsa and Kline (1955), have suggested that combined treatment with reserpine and chlorpromazine is better than treatment with either compound separately, and Lemere (1955) has claimed that the combined drugs tend to cancel out the undesirable side effects of one another. The problem clearly requires further investigation. The question of precise diagnostic indications has not yet been

adequately investigated. Rinaldi, Rudy and Himwich (1956) have studied the comparative action of reserpine, chlorpromazine and Frenquel on chronic schizophrenia and they claim they are effective in that order. However, both reserpine and chlorpromazine are much more active than Frenquel, with reserpine more effective for the catatonic, and chlorpromazine more effective for the paranoid groups.

PACATAL (LACUMIN)

Pacatal is a phenothiazine derivative related to chlorpromazine and has the same basic structure with a different side chain. N-methyl-piperidil-3-methyl in the side chain replaces the di-methylamino-n-propyl chain of chlorpromazine, but without the chlorine atom.

It was synthesized in Germany in 1953 (Schuler and Nezel, 1953; Werenberg, 1955), and tested pharmacologically by Nieschultz *et al.* (1954). In most respects its action parallels that of chlorpromazine, but it has less sedative and less sympathicolytic action. Clinically, it has been used as a potentiator of anaesthetic agents (Horatz, 1954), and as a tranquillizing agent in psychiatry. Werenberg (1955) reports that he treated 100 psychiatric patients, including 86 cases of schizophrenia, 5 of mania and a number of miscellaneous conditions. All had been refractory to other treatments (cardiazol, E.C.T., insulin coma, leucotomy), and some were refractory to chlorpromazine. On a dosage of 50 to 250 mg. by mouth three times daily, 10 schizophrenics and 1 mania were much improved, and 29 schizophrenics improved, making a total of 39 of the 86 schizophrenics either improved or much improved. Fifty-nine per cent. of the patients showed side effects of varying degrees of severity such as dryness of the mouth, vomiting, visual disturbance, loss of appetite, constipation. There was one fatal case of agranulocytosis.

We can now report a small trial on 24 patients who were given either Pacatal by mouth up to 300 mg. per day or inert control tablets. Twenty of these were chronic schizophrenics and refractory to treatments with other drugs. The other 4 were more recent untreated cases and consisted of 3 schizophrenics and 1 agitated depression. The 10 control patients in the chronic schizophrenic group showed no change over a period of 1 month. Those on Pacatal showed 1 marked improvement and 2 moderate improvements in the same period. Of the 4 recent cases, two rapidly improved on Pacatal, while of the 2 controls, 1 improved rapidly, and the other showed only moderate improvement. Severe vomiting occurred in two patients, and visual disturbance due to mydriasis and paralysis of accommodation in a further two sufficiently severe to lead to cessation of treatment. Four patients complained of severe dryness of the mouth. There was no evidence of blood dyscrasia. These results suggest that Pacatal may be effective in patients refractory to other drugs of the type, though it seems that Pacatal may produce more toxic side effects than chlorpromazine. We must stress that this was a small investigation, the clinical material was unpromising, and further experience may lead to a modification of our views.

In assessing the role and future of these drugs, a comparison with the sulphonamides immediately springs to mind. These powerful and toxic drugs were discovered empirically; at first there were some doubts as to their clinical effectiveness, but very quickly the discovery of the rationale of their action and later the discovery of the antibiotics led to a whole new chapter of medicine which has illuminated many aspects of science other than the fairly narrowly defined area where the discovery originated. These new tranquillizers have also

been empirically discovered; they are powerful drugs with pronounced toxicity, and there is still doubt in some quarters as to their clinical effectiveness. However, more and more observers are confirming their beneficial action in certain mental illnesses. More recently clues to their mode of action have been discovered in their effect on diencephalic and brain stem structures, and as serotonin antagonists and activators of the neuroendocrine system. These discoveries are likely to have implications beyond the rather narrow field where they have originated, more particularly in that research pharmacologists and chemists can direct their searches to classes of drugs with actions that can be defined, and there is no doubt that we are facing far reaching developments in the sphere of the pharmacology of the more subtle and complex activities of the central nervous system. We would like to reinforce the plea of Sargent (1956) for careful assessment and clinical testing before these new drugs are generally released to the public. For this some form of co-operative venture might be devised on the lines of the Medical Research Council's Therapeutic Trials Committee, since it is clearly beyond the resources of any individual or single group of individuals to investigate fully all the drugs that are already being produced or all the spheres of activity of a single drug. If, as seems likely, we are on the threshold of another big forward movement in psychiatry, akin to the introduction of insulin and E.C.T., we should make certain that as far as possible the new remedies get full and adequate assessment, so that, for example, the doubts and uncertainties which have dogged insulin coma therapy and which have not even yet been completely resolved, do not recur with these newer remedies.

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DRUGS AND PERSONALITY

I. THEORY AND METHODOLOGY

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INTRODUCTION

FEW people would be inclined to underrate the actual, and, even more, the potential contribution which the study of the effects of drugs on personality can make to psychiatry. The large number of research papers in this field contributed both by psychiatrists and psychologists bears witness to the interest in this field, as does also the testimony of Freud, who is reported by Ernest Jones to have given it as his opinion that in due course pharmacological treatment of psychiatric disorders would oust all others.

When we compare the potential usefulness of drugs with our present knowledge of their effects, and the clinical use made of them at the moment, we cannot but note the wide gap which separates expectancy from achievement. Empirical studies there are many, but they are frequently contradictory and bedevilled by many experimental, statistical and methodological errors. Clinical applications show frequent failures to obtain expected results, and a general difficulty of predicting the reactions of individuals to the given drugs, to say nothing of the eternal problem of dosage. The present position, therefore, cannot be regarded as satisfactory either from the research or the applied point of view.

The writer has elsewhere suggested that personality tests and methods of treatment can be grouped according to whether they derive from notional, empirical, or rational considerations (10). *Notional* in this context is taken to mean a procedure which is based simply on a hunch or a vaguely felt analogy. A procedure is called *empirical* when there is some independent evidence that it does what is claimed for it, although any theoretical background there may be for such a procedure is purely *ad hoc*. A procedure is called *rational* when it derives through a more or less rigorous process of deduction from an empirically supported and theoretically integrated set of postulates, theorems and axioms. The same terms may be applied to the study of drug effects, and it may be claimed that in this field also progress has been delayed by the prevalence of notional and purely empirical studies, and the comparative absence of any rational system which would allow us to predict the effects of groups of drugs, and to test these predictions in terms of the usual hypothetico-deductive methods of science. In the present paper the writer has made an effort to provide such a theory and to discuss certain relevant aspects of methodology and experimental design; subsequent papers will deal with experiments conducted in an attempt to test deductions made from theory.

* The work reported in this and the following papers was made possible by a grant from the Bethlem Royal Hospital and Maudsley Hospital Research Committee. Thanks are also due to Dr. C. M. Franks and Dr. D. Trouton for permission to quote from their unpublished work on the effect of drugs on conditioning, and to Dr. C. Shagass and the Pergamon Press for permission to quote and reproduce the figures from his paper on the sedation threshold.

MCDougALL'S CHEMICAL THEORY OF TEMPERAMENT

The theory which will be proposed here has certain important similarities to one proposed almost 30 years ago by William McDougall (17) and, as a comparison of these two theories will be instructive, a brief outline of McDougall's hypothesis may not be out of place. He began by accepting the existence of a continuum or dimension of personality corresponding to Jung's factor of extraversion-introversion: "I suggest that all personalities can be arranged in a single linear scale according to the degree to which this factor is present in their constitution . . . such a distribution of a temperamental trait is most naturally explained by the influence of some one chemical factor generated in the body and exerting a specific influence upon the nervous system in proportion to the quantity that is produced and liberated into the blood stream." McDougall confessed himself puzzled by the problem of which was the positive and which the negative state. He finally came to a somewhat arbitrary decision: "In all probability extraversion is the positive state, introversion the negative, that is to say, extreme introversion represents a defect, a minimal quantity or minimum rate of secretion of the postulated substance—(let us call it X); and extraversion in its various degrees is a consequence of correspondingly large quantities or rapid rates of secretion of X." How is this secretion supposed to act? McDougall begins by explaining his theory of introversion: "The introvert . . . is the man in whom the lower levels of the nervous system are constantly subjected to a high degree of inhibition by the higher cortical activities, and of the lower inhibited functions the most important are the affective or emotional-conative functions of the thalamic region . . . Thus the introvert, by reason of the predominant activity of his cortex and in virtue of its restraining or inhibitory effect on the outflow of thalamic excitation in its normal or direct channels of emotional expression, is a man in whom thought seems to flourish at the expense of emotion . . . introversion seems then to be the natural consequence of the great development and free activity of the cortex."

McDougall then goes on to postulate that there is an increase in introversion as children grow up into adults corresponding to the greater functional dominance of the cortex. However, "Nature has provided an antidote against such increasing and excessive introversion. It has generated in the tissues, or in some tissue unknown, an extraverting hormone, or endocrine substance, the function of which is to prevent, to diminish in some measure this inhibiting, paralysing influence of the cortex upon the more primitive, lower level functions of the nervous system. The man who is constitutionally provided with a large amount of this antidote to cortical inhibition is the extravert."

We now come to the pharmacological aspect of McDougall's theory. "How . . . may we conceive the postulated internal secretion X to work upon the brain to maintain various degrees of extroversion, to antagonize and moderate the inhibiting influence of the cortex? I suggest that we may find the clue to a simple, intelligible and adequate hypothesis in consideration of the influence of alcohol upon the brain functions (and of ether and chloroform), and that the phenomena of alcoholic intoxication go very far to justify the hypothesis. . . . I have observed in a number of cases that the markedly extraverted personality is very susceptible to the influence of alcohol. A very small dose deprives him of self-restraint and control and brings on the symptoms of intoxication, all of which are essentially expressions of diminished cortical control over the lower brain levels. The introvert, on the other hand, is much more resistant to alcohol. He can take a considerable dose without other effect

than that he becomes extraverted . . . Alcohol, in short, seems to be an extraverting drug pure and simple so far as its influence on the nervous system is concerned."

McDougall now formally states his hypothesis. "In order to explain extraversion, I make, then, the simple assumption that in the extravert some tissue (or tissues) normally and constantly secretes the extraverting substance X, a substance whose action upon the nervous system is very similar to that of alcohol (ether and chloroform); that is to say, I assume that the extraverting internal secretion X acts directly upon all synapses, raising their resistance to the passage of the nervous current or discharge from neurone to neurone. I make also the highly probable assumption that the synapses of the various levels of the nervous system are in the main solidly and stably organized in proportion to the phylogenetic and ontogenetic age of the levels in which they occur. In other words, I assume that the synapses of the high levels are the less solidly organized, have higher resting resistances, and are less stable, more subject to variations of their resistance by a variety of influences, including the chemical ones of strychnine, alcohol, and the postulated substance X."

Having stated this formal hypothesis, McDougall goes on to explore some of the theoretical connections with abnormal mental states. He argues that extraverts are more prone to hysteria, hypnosis, trances, automatic actions, crystal visions and so on, while introverts are more prone to neurasthenia, schizophrenia and insomnia. "All these differences seem to mean the greater liability of the extravert to suffer dissociative effects in the nervous system, whether local, as in local functional paralyses and anaesthesias, or general, as in general amnesia, trance, hypnosis and sleep. And this is to be expected; for, just as alcohol is a dissociative drug, which acts first and most intensely upon those most delicately organized synapses that are involved in the latest acquired and highest-level processes of the cortex, subserving self-conscious control and self-criticism, and involving the reciprocal play of one cortical system of highest level neurones upon another, so also the extraverting substance X may be supposed to affect most markedly these higher level synapses, maintaining during waking life an incipient state of dissociation and rendering easier the onset of all more pronounced states of cerebral dissociation, from normal sleep and alcoholic intoxication to hypnosis and functional paralyses and amnesias. In short, the introvert is liable to disorders of continuing conflict, because conflict cannot readily be obviated by dissociation; while the extravert readily finds relief from internal conflict through the onset of some complete dissociation between conflicting systems and tendencies."

This, then, is a brief outline of McDougall's hypothesis, which to the writer seems plausible, ingenious and extremely fruitful. It has been almost completely neglected by psychologists and psychiatrists alike for a variety of reasons, some of which may be worth stating. In the first place, McDougall does not provide objective measures of extraversion-introversion which might be used to identify any given person's position on the continuum, and which might be used to measure the shift of that person's position on the continuum subsequent to the administration of the given drug. His argument is confined to observational methods whose unreliability was becoming more and more apparent in the 1930's as a result of psychological investigation. As we shall have occasion to point out later in connection with the methodology of drug research, objective methods are absolutely essential in the testing of a hypothesis such as that advocated by McDougall. Thus the work of Shagass (21, 22) is a striking confirmation of McDougall's observation quoted above that "the markedly extraverted personality is very susceptible to the influence of alcohol" while "the

introvert, on the other hand, is much more resistant to alcohol. He can take a considerable dose without other effect than that he becomes extraverted." But this demonstration had to await the elaboration by Shagass (21, 22) of an objective method for measuring the intoxication threshold, or "sedation threshold" as he calls it.

The second reason for the failure of McDougall's hypothesis to be widely accepted lies in the mysterious nature of his substance X. This appears purely *ad hoc*, is not integrated in any way with the existing body of psychological knowledge, and does not conform with the rules science lays down for the introduction of hypothetical constructs and intervening variables. In the third place, McDougall was rather half-hearted in his specification of the relation between temperament and drugs. His main interest obviously being in substance X, drugs are only introduced into the paper by virtue of the hypothetical similarity in action between alcohol and X. A last reason may be found in the general tendency of psychologists at the time McDougall's article was written to disown higher-order concepts implying generality in the personality field (such as extraversion-introversion), and to seek instead for specific stimulus-response connections (5). All these reasons working together may explain, but cannot justify, neglect of McDougall's contribution.

THE DRUG ACTION POSTULATE

Much of the present writer's work in the experimental field has been devoted to an attempt to fill in the gaps in McDougall's theory. Thus a certain amount of effort has been applied to the formal proof of the existence of a personality dimension analogous to the concept of introversion-extraversion as advocated by Jung and McDougall (3, 4, 5). This has been accomplished in terms of objective tests which can be used as an operational definition of this dimension. The success which has accompanied this demonstration suggests the possibility of using tests of this type in a verification of deductions made from any theory of drug effects on extraversion-introversion.

The writer has also attempted to integrate McDougall's mysterious substance X with the general body of modern psychological theory by postulating that, as Pavlov had already suggested, personality differences between extraverts and introverts are mainly due to disturbances in the cortical excitation/inhibition balance, in the sense that extraverted behaviour patterns are produced by excessively strong reactive inhibition and/or excessively weak excitation, while introverted behaviour patterns are produced by excessively weak reactive inhibition and/or excessively strong excitation (6, 7). The terms excitation and inhibition in this connection are molar concepts clearly defined in the systematic writings of Pavlov and, more particularly, of Hull; therefore, their use should not be understood to imply any physiological hypothesis, although the recent work of Eccles (1) has given some hope that a physiological and neurological substructure for these molar psychological concepts may yet be achieved.

In thus bringing into line the major concepts of modern learning theory and of personality theory, the writer was also able to add one of the prominent groups of data from the study of perception by demonstrating that satiation phenomena as demonstrated by Köhler, can be explained in terms of the same inhibitory mechanisms postulated above. Experimental evidence regarding this whole scheme has been supplied in a number of recent publications which should be consulted by those interested in the general theory of extraversion-introversion (8, 9, 11, 12). Here only two further points will be noticed.

In the first place it was demonstrated, as had already been postulated by Jung and McDougall, that hysterics and psychopaths show a strong tendency to be extraverted in their behaviour and their test scores, while dysthymics tend to be introverted in their behaviour and their test scores (3, 4, 5). (The term "dysthymic" was introduced specifically by the writer to cover the neurotic syndrome characterized by manifest anxiety, reactive depression and/or obsessive compulsive symptoms. It was introduced because of the uncertain meaning and obsolete nature of the terms "psychasthenia" and "neurasthenia" sometimes used by older writers in this connection.)

In the second place it was demonstrated that brain damage in general, and, particularly, damage to areas 9 and 10 in the frontal part of the cortex, had an extraverting effect on behaviour, and produced a general increase in cortical inhibition in the individuals operated on or subjected to accidental damage (6, 18, 19, 20). This finding is of course in line with McDougall's conception of the cortex as an inhibiting centre for lower level activities; the damage (inhibition) of an inhibiting organ has precisely the effect of his postulated factor X.

The link between drug action on the one hand and learning theory on the other (and therefore through learning theory with personality theory, as briefly indicated above) was adumbrated by Hull (15) in a pioneer paper on the influence of caffeine on rote learning. Basing himself on a quotation from C. L. Evans (2) to the effect that caffeine had a marked tendency to eliminate internal inhibition, Hull made certain predictions on the molar effects which should follow in human rote learning from the administration of this drug. Thus, he argued that if, as Lepley (16) had shown, the bowing of the serial learning curve is due to internal inhibition, then the administration of caffeine, by eliminating such inhibition, would reduce the degree of bowing observed. Working on only eight subjects, he failed to find the predicted effect, but found instead that "the subjects as a group showed a fairly definite tendency to give more anticipatory reactions after taking caffeine, the mean percentage of increase being 33, with a probable error of 7 and a satisfactory critical ratio of 4.7". This effect can be predicted from the hypothesis that caffeine eliminates internal inhibition; as anticipatory reactions are supposed to be held in check by these inhibitions, they would therefore be released by their elimination. However, Hull appears to have regarded this experiment as a failure, and never returned himself to this vital area.

We are now in a position to put in the form of a postulate the expanded theory adumbrated by McDougall and Hull. This postulate reads as follows: *Depressant drugs increase cortical inhibition, decrease cortical excitation and thereby produce extraverted behaviour patterns. Stimulant drugs decrease cortical inhibition, increase cortical excitation and thereby produce introverted behaviour patterns.* This postulate was informally suggested in a previous paper, but has not hitherto been stated in any formal way (6). It should be noted that the terms "depressant" and "stimulant" are here used in their pharmacological sense as listed by Goodman and Gilman (14); it would be valuable to know the chemical and biochemical properties characterizing these drugs and causing the opposing effects, but such knowledge does not appear at present to be available. It should also be noted that in many cases drugs which fall in one or the other of these two groups have side effects which may be so strong as to cancel out the predicted effects; thus many excitant drugs are also sympathicomimetic. It is important in submitting the postulate to experimental investigation to choose drugs having as few side effects as possible. We shall refer to this problem later on in this paper.

We may now note some of the ways in which the present theory differs from, and may be said to improve upon, that of McDougall. In the first place, the drug action postulated is stated in terms of two reasonably well defined groups of drugs whose existence is clearly recognized by pharmacologists and whose contradictory properties are well known. In the second place, the effects postulated can now be deduced directly from a general theory of behaviour. This point can be illustrated by reference to Figure 1 which shows the postulated correspondence between drug effects and the various levels of our behaviour-personality theory. These relationships should be borne in mind during our discussion of the methodology of drug research which forms the next section of this paper, and which amplifies this point in considerable detail. In the third place, we do not have to rely, as did McDougall, on simple observation, but are enabled to test our deductions with reference to objective laboratory procedures in the fields of learning and conditioning, of perception, and of the work decrement. These are important advantages, as by making predictions more precise and more objective, we also make the theory more incisive, and easier to disprove if in error.

Causal level:	Excitation—Inhibition.
Clinical-behavioural level:	Dysthymia—Hysteria.
Test level:	Introversion—Extraversion.
	Stimulant—Depressant.

FIG. 1.—Drug effects and three levels of investigation proposed in the text.

METHODOLOGY OF DRUG RESEARCH

The success or failure of any theory depends essentially upon the ability of that theory to enable investigators to make clear-cut deductions and testable predictions. It is part of the duty of anyone proposing such a theory to indicate the types of prediction which he would regard as crucial to the correctness or incorrectness of his theory. It is this task to which we must now turn.

Roughly speaking, we have open to us three avenues corresponding to the three levels (causal, clinical, and test level) indicated in Figure 1. Thus at the clinical-behavioural level our theory would imply the prediction that *stimulant drugs produce dysthymic symptoms and behaviour patterns, and a reduction in hysterical symptoms and behaviour patterns. Conversely depressant drugs produce an increase in hysterical symptoms and behaviour patterns, and a decrease in dysthymic symptoms and behaviour patterns.* This is probably the least useful type of prediction because of the great observational difficulties which impede objective determination of the predicted results.

At the test level, we would predict that *any test which has been shown to differentiate reliably and validly between introverts and extraverts will, when applied to subjects who have been administered a stimulant (or depressant) drug, show shifts in scores in the direction characteristic of greater introversion (or extraversion).* In view of the large number of tests covered by this deduction and the objective nature of the scores obtained, this is a useful and valuable type of prediction. We might add to it, in parentheses, a similar prediction derived from the fact that brain injury appears to have extraverting consequences, namely, that *the effects of depressant drugs are similar to those of brain damage as far as objective psychological tests are concerned. Conversely, the effects of stimulant drugs are opposite to those of brain damage.*

At the third level, and this may be regarded as the most fundamental of all, we are dealing with the hypothetical causal factors underlying both clinical behaviour and test scores. Our predictions here would therefore be in terms of

the theory of excitation-inhibition, and can be applied directly to the field of drug studies without necessarily going through the intermediate state of being applied to the dysthymic-hysteric differentiation, or that between extraverts and introverts. (Hull's (15) prediction, mentioned above, would be of this type.)

While the last type of prediction is the most advanced theoretically, and the most fundamental psychologically, it is also probably the one presenting the greatest difficulties. The theory of excitation and inhibition is nothing like as rigorous, definite and clear-cut as one would like it to be, and the failure of an experiment to satisfy the drug postulate may be due to a mistaken application of the general behaviour theory, rather than to an error in the drug postulate itself. This of course should not deter the investigator from making predictions of this kind and testing them regardless of the outcome. The results would almost certainly be of considerable interest from the point of view of learning theory, as well as from that of the study of drug effects. Indeed, one of the most important outcomes of the study of drug effects might be a clarification of certain puzzling features in learning theory, and the growth of a methodology for verifying or disproving certain assumptions of learning theory.

Nevertheless, from the point of view of making predictions which are crucial for our postulate, it would seem advisable to choose tests whose use can be justified at all three levels, i.e. tests whose theoretical derivation at the causal level is clear cut, which are known to differentiate between dysthymics and hysterics, which are known to distinguish between normal introverts and normal extraverts, and which are known to be affected by brain damage in a certain manner. An example of such a procedure is that of conditioning. Ease of conditioning is clearly determined by the growth of excitatory potential and the relative absence of inhibitory potential, while difficulty in the forming of conditioned reflexes is clearly related to the presence of strong inhibitory potential and the relative weakness of excitatory potential. Thus, on the causal level conditioning techniques present as clear-cut a prediction as we can make.

At the other two levels, work in our laboratory (11, 12) has clearly shown that eyeblink and PGR conditioning differentiate at a very high level of reliability and validity between hysterics and dysthymics, and between extraverts and introverts. Similarly, there is in the literature a good deal of evidence to the effect that brain operations tend to have an inhibitory effect on conditioning. All in all, then, the results from these various sources indicate that if our postulate is correct, *depressant drugs should produce a decrease in the rate of conditioning, while stimulant drugs should produce an increase in the rate of conditioning.*

The evidence regarding this prediction has been reviewed by Franks and Trouton (13), who have also performed an experiment using eyeblink conditioning as the dependent variable. By random allocation of subjects they made up three groups, one of which, the control group, received a placebo, while the other two received sodium amytal and dexedrine respectively. The results were very definite and in conformity with our hypothesis as will be seen from Figure 2. The group which had been administered the sodium amytal conditioned least well, the group which had been administered the dexedrine conditioned best of all, while the group which had been administered the placebo was intermediate. This experiment may serve as an example of the type of prediction which can be made with the greatest confidence from our postulate.

The research design used by Franks and Trouton has been illustrated in

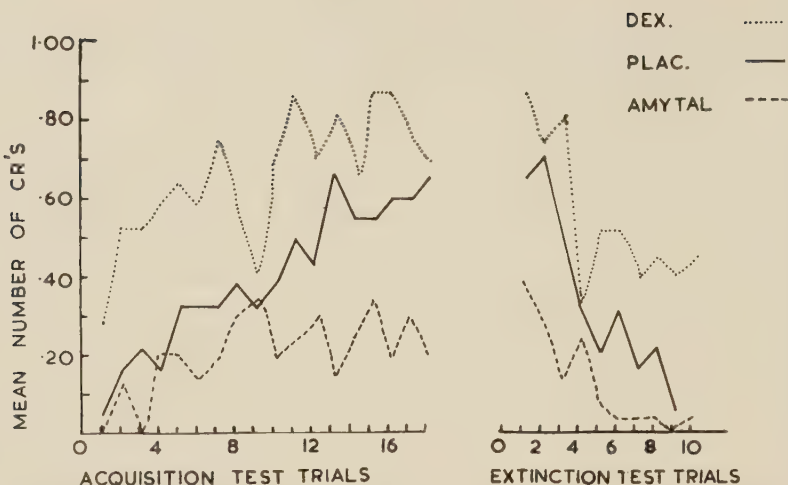


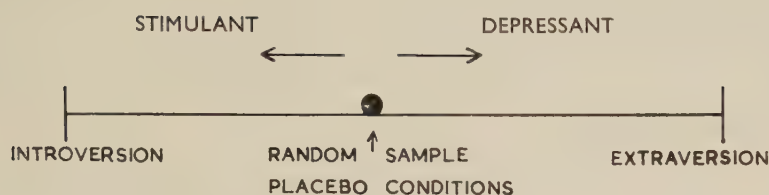
FIG. 2.—The rate of conditioning and extinction under dexedrine, placebo, and sodium amytal

Figure 3a. It will be seen that it does not depend in any way on the assessment of the personality of the subjects prior to the experiment. Subjects are randomly allocated to control and experimental groups, and what is studied is the *general* effect of the drugs under investigation on what might be called the *standard subject*. This paradigm is of course capable of certain improvements. Thus the same group of persons might be tested three times under placebo, depressant and stimulant drug conditions, so that each subject would constitute his own control. This is possible in perceptual experiments, but not in conditioning and learning experiments where nearly all the improvement in performance takes place during the first session. Other improvements in the experimental design contingent upon the one suggested above might include the assessment of the standing of the subjects on the extraversion-introversion continuum, and the calculation of possible interaction effects in an analysis of variance design. However, these refinements do not in any way affect the general principle of this design, which is probably the most widely used of all.

A rather different design is illustrated in Figure 3b. This design makes use of the known position of groups of subjects such as dysthymics and hysterics on the introversion-extraversion continuum, and thus explicitly contravenes the random sampling technique of design A. This design harks back to McDougall's observation that extraverts need less alcohol to reach a point of intoxication than introverts, who with the same amount of alcohol simply become more extraverted. Such a research design requires an objective *terminus ad quem*, i.e. the terminus of an "intoxication threshold" which would enable us to ascertain the amount of alcohol required by different groups of subjects to reach the same level of cortical inhibition as defined by this threshold.

The only example of the use of such a technique which the writer has been able to find is a study by Shagass (21, 22) using what he calls the "sedation threshold" of sodium amytal. The sedation threshold is an objective pharmacological determination, which depends on EEG and speech changes produced by intravenously given amylo-barbitone (amytal) sodium. "Amylo-barbitone sodium is given intravenously at the rate of 0.5 mg./kg. of body weight every 40 seconds. The patient is tested for slurred speech, and the injection is continued at least

A



B

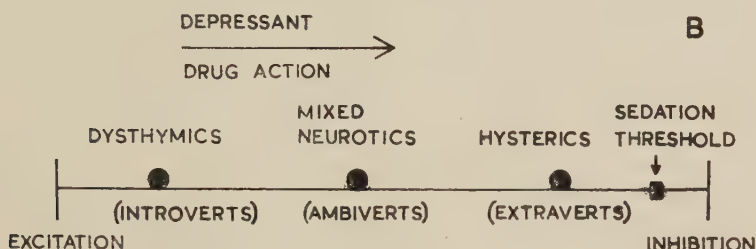


FIG. 3.—Two fundamental research designs for the study of drug effects.

80 seconds after slurred speech is noted. Continuous EEG's are recorded from transverse frontal and sagittal frontocentral placements." Figure 4 shows that sodium amytal produces a rather striking increase of fast frequency (15–30 c.p.s. activity). The amplitude of this fast frequency is taken by Shagass as a

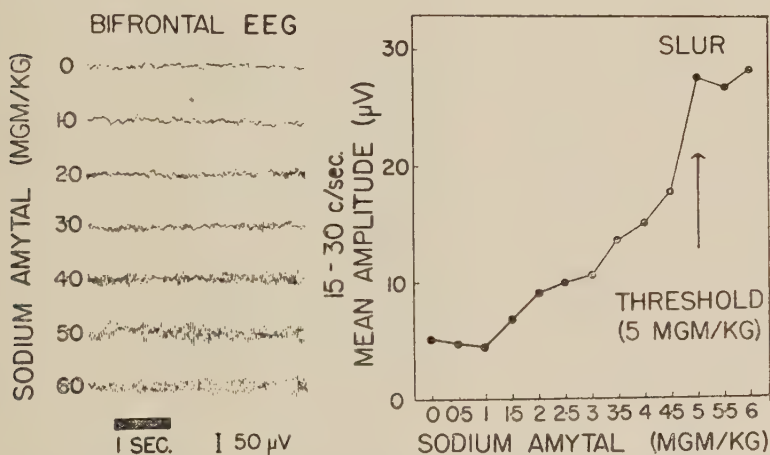


FIG. 4.—Effect of sodium amytal on bifrontal EEG. Note progressive increase in fast frequency amplitude. Arrow points to inflexion point in the amplitude curve, which indicates sedation threshold.

response to the drug and the dosage-response curve plotted. "The typical curve has a sigmoid shape and contains a point of inflexion, preceding which there is a sudden increase in the amplitude of the fast activity, and following which the curve tends to plateau. This inflexion point generally occurs within 40 seconds (0.5 mg./kg.) of the time when slurred speech is first noted, and the slur and inflexion point are used together as indicators of the threshold. The threshold

is the amount of sodium amytal, in mg./kg., required to produce an inflexion point in the 15–30 c/seconds amplitude curve, which occurs within 80 seconds (1 mg./kg.) of the time when the slur is noted. The slur localizes the threshold roughly, the EEG inflexion point does it more precisely . . . The measurement is highly reliable; its probable error is no greater than 0.5 mg./kg. of body weight. Age, sex, and previous intake of sedatives in usual psychiatric dosage have not been found to influence the threshold."

According to the theory outlined in this paper (which was developed before Shagass's work was known to the writer), we should be able to make a very definite prediction. Sodium amytal, being a depressant drug, would be postulated to increase inhibition. An extravert, whose cortex, according to our theory, is already in a relatively inhibited state, should require comparatively little sodium amytal before reaching the critical sedation point; such a person should have a low sedation threshold. The introvert, on the other hand, whose cortex is in a state of considerable excitation and low inhibition, would require a considerable amount of sodium amytal before reaching the critical sedation point; he would be predicted to have a high sedation threshold. If we express this general hypothesis in terms of neurotic groups and their standing on the extraversion-introversion continuum, then we would expect psychopaths to have the lowest threshold, followed by hysterics. Mixed neurotics would be intermediate and anxiety states, obsessionals and reactive depressives would have high sedation thresholds. An experiment along these lines was carried out by Shagass; his results are given in Figure 5. It will be seen that these results bear out our prediction in every detail.

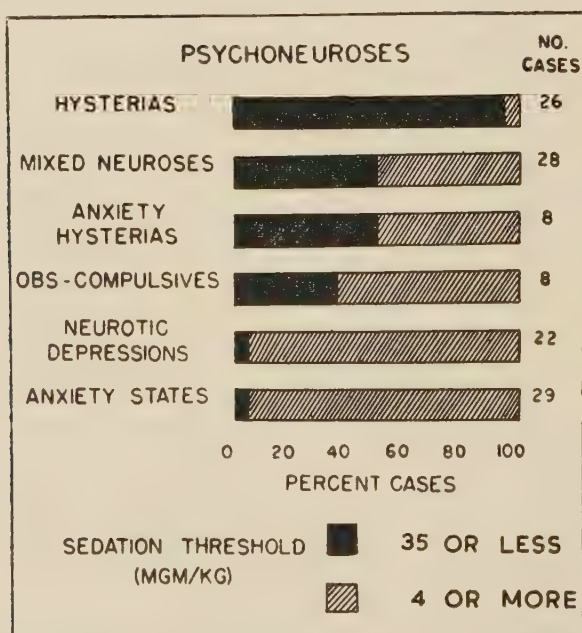


FIG. 5.—Differentiation between psychoneurotic groups by a division point between thresholds of 3.5 and 4.0 mg./kg.

This design too, of course, is capable of certain interesting modifications. If something corresponding to the sedation or intoxication threshold could be found at the introverted end of the continuum, we would predict lower thresholds

for dysthymics than for hysterics, and quite generally a reversal of the relations found on Shagass's research. Thus the same groups of patients at different times might be given different and opposing drugs as well as placebos. However, such developments depend on the discovery of such threshold effects for excitation drugs, which might possibly be looked for in EEG patterns corresponding to wakefulness as opposed to sleep.

DRUG STUDY AND BEHAVIOUR THEORY

The practical usefulness of the successful elaboration of a theory of drug action on personality will be too obvious to require any further comment. The writer would like to stress, however, the point already mentioned in a previous section, namely the reciprocal interaction between a theory like that outlined here and general behaviour theory. It is customary in the work of Hull, Pavlov, Spence, and their followers to attribute certain psychological effects to hypothetical constructs and intervening variables like excitation or inhibition, without proving that the inhibition responsible for, say, reminiscence effects is the same inhibition which is responsible for, say, serial learning position effects. While it is very likely that phenomena which obey the same laws do, in fact, depend upon the same hypothetical constructs, a more formal demonstration or proof would seem to be required. Such a proof, in the writer's opinion, can be furnished most easily by reference to the field of individual differences and drug effects.

This general view can perhaps be illustrated best by reference to a hypothetical example. Let us suppose that two experimental phenomena, A and B, are supposed to be produced by the mechanism of inhibition. One method of proving that this was so would be to demonstrate on the physiological-neurological level that the underlying processes in the central nervous system were identical. Such direct proof would of course be the most satisfactory method of dealing with this problem, but unfortunately the possibility of such a demonstration is extremely remote at the present time. Consequently, we must look for some other method of proof.

To aid us, we have two postulates, which include the factor of inhibition in a manner experimentally independent of phenomena A and B. We have, first of all, the temperamental postulate stating that inhibition is stronger in extraverts than in introverts, and we have the drug postulate stating that depressant drugs increase inhibition whereas stimulant drugs decrease inhibition. Proof of these two postulates lies in tying them up with phenomena C, D, E . . . N, which form part of the general inhibition theory. We can now apply these postulates to phenomena A and B and state that if these *are* phenomena produced by the general factor of inhibition, then (a) both A and B should be more pronounced in extraverts than in introverts; (b) both A and B should be more pronounced after the administration of a depressant drug than after the administration of a placebo; and (c) both A and B should be weakened after the administration of a stimulant drug as compared with the administration of a placebo. These are testable predictions which enable us to answer our original question regarding the status of phenomena A and B in learning theory, and they at the same time give us information regarding the status of phenomena A and B within personality theory, and within the theory of drug effects. Thus, this mutual interweaving of data from different and hitherto largely isolated fields within the general field of psychology constitutes the main claim of the temperamental and drug postulates to the attention of psychologists and psychiatrists interested

in fundamental theory and in the integration of the biological sciences concerned with the study of human behaviour.

These mutual interactions may also be used to solve certain problems in the pharmacological field. Thus it is often found that drugs have very diverse effects on people, so that one and the same dose of a drug given to two people may produce an apparently strong effect in the one and almost no effect in the other. Altogether it has been found impossible to rationalize the amount of drug which ought to be given in order to produce similar results. Pharmacologists of course have worked out certain rules. There is, for instance, the theory of what is called the "therapeutic ratio", which is obtained by dividing the lethal dose by the therapeutic dose. Then there are such rules as those of Clark, Young, and Cowling, relating to age, and the various rules relating dosage to body weight (14). In practice these rules appear to have very little value, particularly where stimulant and depressant drugs are concerned.

The reason for this follows directly from our postulate system. It appears obvious that in terms of that system, the most important variable in predicting the effects of the drug, and in prescribing the particular dosage required for a specific purpose, would be the excitation-inhibition ratio obtaining within the particular person concerned. This ratio would not be likely to be very highly correlated with weight or any of the obvious surface characteristics which are taken into account by pharmacologists at present. Thus, to provide a given effect, such as the reaching of the sodium amytal sedation threshold, the dosage required is clearly related to the position of the subject on the extraversion-introversion continuum. That position, as Shagass's results show, correlates far more highly with the dose needed than does any of the variables at present used by pharmacologists to assess the amount of drug required. It is likely that we can explain in a similar way the differential effects of various doses of alcohol. It is also likely that the same rule would apply in the opposite direction, such that introverts would require relatively little dexedrine or caffeine to reach a level of excitation (sleeplessness?) which could only be reached by extraverts after a considerably larger dose of these drugs had been administered.

It will be seen then that the theory here proposed makes possible the beginnings of a rational solution to a number of problems in drug administration. While the theory is not in any sense quantitative as yet, there is no reason to expect that if our hypotheses have been at all along the right lines, such quantification should prove difficult or impossible. In the writer's view, the next point of advance in the study of the relationship between drugs and personality will be that of developing truly quantitative relationships on the rational basis provided.

SUMMARY

A theory has been developed in this paper linking the action of depressant and stimulant drugs with increases in cortical inhibition and cortical excitation respectively. These terms are used in the sense given to them by Pavlov and Hull, and thus the theory proposed links up modern behaviour theory with drug action. As a further step, the personality dimension of extraversion-introversion (hysteria-dysthymia) is introduced by a postulate, formally stated in a previous paper, relating strong excitatory potential to introversion and dysthymia, and strong inhibitory potential to extraversion and hysteria.

A discussion is given of the possible range of deductions which can be made from the theory suggested, and some confirmatory experiments are quoted, dealing with conditioning and the sedation threshold. A detailed discussion is also given of methodological problems arising from attempts to verify deductions from theories such as the one proposed.

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INITIAL PSYCHIATRIC ILLNESS IN INVOLUTIONAL WOMEN

I. CLINICAL ASPECTS*

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INTRODUCTION

THE diagnostic criteria of descriptive psychiatry inevitably vary between the generations and even from decade to decade. New hypotheses imply new selections from the quite illimitable mass of data which may be collected from any patient. Hence new pictures may emerge whose relation to the old may be only partial, and whose inclusion under traditional rubrics may give rise to confusion, or pose problems which are verbal rather than clinical.

Even without any formal hypotheses, however, other factors combine to produce a vexing variation in diagnosis. New techniques of examination, psychological and physical, offer the possibility of hitherto undiscerned classifications appearing, as it were, spontaneously. The development of more adequate statistical methods displays some of the limitations of earlier samples, and points the way to ever more precise surveys. Inasmuch as the content of secondary psychotic symptoms may be effectively influenced by cultural changes, constant re-assessment of symptomatic material may point to what is essential, and what is not, in a classical picture. The impact of effective therapy may stress earlier diagnosis, and therefore give a population of cases showing not only a modified symptom distribution but perhaps also including individuals who would never before have achieved diagnostic recognition. Therapy, again, may perplexingly cut across diagnostic boundaries, so that many clinicians now tend in practice to assess their patients more in terms of potential therapeutic response than in those of orthodox diagnosis. A viewpoint of this sort achieves a formal dynamic recognition in such textbooks as those of Sargant and Slater (1954) in this country, and Leo Alexander (1953) in America. Such therapeutic approaches do not, of course, necessarily imply the abandonment of the attempt to achieve some kind of taxonomic order. Indeed, having available a new operative dimension of unnatural history may conceivably end in a more fruitful classification than hitherto.

This recent multiplication of vantage points, from which one may survey clinical material traditionally arranged, has therefore blurred many of the hallowed margins of psychiatric nosology. Rather than retreat, however, into the anarchy of over-emphasizing the uniqueness of the individual, we may simply consider that a frequent revision of clinical data still has its place in psychiatric research, accepting the dictum of Mayer-Gross, Slater and Roth (1954) that "Description of phenomenologic aspects admittedly is not a prime

* Some of this material was included in a paper read by one of the authors (A.C.T.) at a Quarterly Meeting of the R.M.P.A. on 17 May, 1956.

interest in most schools of psychiatry today, but it would be well if it excited more interest than it does”.

The considerations mentioned above apply with particular force to the concept of involutional melancholia. For example, one former distinguishing mark was of a duration likely to be longer than that of an earlier depressive episode, as in the average $9\frac{1}{2}$ months, if recovery were to take place at all, of Hoch and MacCurdy (1922), and the minimal 6–9 months of Henderson and Gillespie (1940). Bleuler (1923), who perhaps wisely gave only a page of his textbook to the subject, said that they grow very slowly, readily remain in their height for several years, and again require a long time to decline. Yet today duration with treatment is if anything said to be shorter than that of a manic-depressive episode treated with E.C.T.

Other obvious changes might be cited, but their discussion is more appropriately left to later parts of this paper. It is sufficient to say that while most of us still carry in our minds the Gestalt of a classical involutional melancholia, we may confess that such pictures are increasingly rare, or alternatively are swamped by the larger mass of mild depressions of the involution which therapy has given us the encouragement to discover. Such depressions form a considerable part of mental hospital practice, and it was felt that a further study of their nature might be of profit.

THE CONCEPT OF INVOLUTIONAL MELANCHOLIA

More perhaps than most psychiatric entities, involutional melancholia has meant many things to many writers. Its very legitimacy, as an aetiological or symptomatic whole, has indeed been suspect since shortly after its birth. In 1896, Kraepelin asserted its independence from periodic insanity, not only symptomatically, but also as an acquired rather than a constitutional disorder. This concept of agitated melancholia was soon attacked on the grounds that the picture was that of a mixed manic-depressive syndrome, and the criticisms of such writers as Dreyfus (1907) were ultimately accepted by Kraepelin. Dreyfus's re-examination of Kraepelin's material was, however, itself the subject of much later criticism by British and American authors, on the grounds mainly of incompleteness and of special pleading in emphasizing very doubtful manic-depressive features. Meanwhile, in Germany, the development of the argument was closely related to matters of theory: it is discussed in the historical review by Lewis (1934a).

It is not here proposed to discuss in full the more recent literature regarding involutional depressions. *Pari passu* with the retention of older criteria, some authors have tended to expand their heading to include, for example, what might formerly have been regarded as climacteric neuroses. Complete comparison of individual papers thus requires an exegetic detail inappropriate to this report, and in any event, the survey by Bellak (1952) covers much of the ground. In order, however, to make intelligible our choice of certain data for clinical assessment it is desirable to sketch a general outline of current descriptions of involutional melancholia, and sometimes to amplify this outline by reference to work which illuminates its source or qualifies its likelihood.

If, therefore, one might make a kind of amalgam of textbook teaching on this subject, it would provide a series of defining statements rather of this sort.

1. Involutional melancholia probably exists as a distinct entity, with such potential misgivings as Henderson's Meyerian comment that it is a convenient label for a certain group of cases, where some specified characteristics are unusually common, or as Mayer-Gross's reservation that affective involutional

psychoses are a somewhat heterogeneous group, but that their several kinds are not easily distinguished clinically.

2. It is largely agreed that the term be reserved for cases not formerly showing any affective illness, and therefore that second or later attacks of depression should be excluded from the concept. It seems obvious that if involutional melancholia is to be regarded as a meaningful entity, and not merely as a pathoplastic variant of former attacks, such cases must in fact be excluded. But it is worth remembering that these exclusions did not always operate, and a number of authors, whose papers have in other ways influenced the textbooks, failed to eliminate from their case material patients who had previously suffered manic-depressive attacks. Thus in Hoch and MacCurdy's differentiation of recovered and unrecovered cases, a scrutiny of their case protocols shows that 12 patients were described as typically manic-depressive; and, although this is not reflected in the text of the article, eleven of these appeared in their recovered group, thus influencing their prognostic criteria, which in turn are still echoed as of general application in some textbooks.

It is generally stated further that cases of initial manic-depressive type should also be excluded. Obviously this must be either a post hoc consideration, in terms of future repetition of illness or the development of cyclical forms, or alternatively it must rest entirely on the further assumption that the entities are symptomatically distinguishable.

3. The next statement, then, in this traditional picture is that the symptomatology of the involutional melancholic differs from that of his manic-depressive confrère. Firstly, one may consider differences in the quality of the essentially depressive symptoms. Here the nature of variation is usually said mainly to lie in the several directions of apprehension or anxiety or agitation as opposed to retardation, of self-reproach or ideas of guilt being unduly prominent, of a hypochondriasis which may be grossly bizarre, and of delusions which may be nihilistic, and even grandiose in their nihilism. Most modern authors, however, implicitly or explicitly regard these symptoms only as frequent condensations or aggregates in the involutional period, and often cite qualifying reservations that such pictures can be seen, even not uncommonly, in younger patients.

The kernel of the matter seems to be to arrive at a definite statement as to whether their occurrence is in fact significantly greater in the involutional period. Some relevant material is available: we would refer mainly to the most considerable clinical study of depression in English, that of Lewis (1934b). In the discussion of his clinical material, Lewis not infrequently draws attention to the occurrence of so-called involutional features in younger patients, although admittedly he, in any event, regards such distinctions as unsophisticated. Unfortunately his material is not tabulated and no mathematical statement is made. However, by using the case protocols in his later paper on prognosis (Lewis, 1936), and identifying by these numbered records the case data of 1934, it is possible to construct an age tabulation relating to those patients over and under 40, and to correlate symptomatology with these groupings. From such a tabulation, it is evident that self-reproach was equally distributed between the two groups; that hypochondriasis, and often most bizarre hypochondriasis, was if anything commoner in the young depressive. It was true that agitation showed some bias to the older, and retardation to the younger, age groups, but none of these biases, whether in the expected direction or not, approached significance. Jarvie (1950) records a divergent incidence of retardation and what he describes as "anxiety" as between patients under and over 45; each attribute,

nevertheless, occurs in both groups. Anderson (1936), in his paper on the prognosis of depressions in later life, said for example of agitation that it was difficult to be sure that there was anything which did not occur in cases of depression of all ages.

One should also, perhaps, be wary of ascribing too much significance to the content of secondary psychotic symptoms. Cultural and historical fluctuations in schizophrenic delusions are well known, but von Orelli (1954) has recently drawn attention to changes in the content of depressive ideas in melancholia, assessed from records covering a period of seventy-five years.

Secondly, a number of other clinical attributes, not usually regarded as primarily depressive, are described as belonging to the involutional picture. Recurring statements mention the relatively frequent occurrence of prominent hallucinations, of clearly paranoid elements, of shallowness of affect, of mildly aggressive whining peevishness, and of schizophrenic admixtures in the symptomatology. Here it is worth remembering that Hoch and MacCurdy, for example, discuss quite explicitly the possibility that their unrecovered cases of so-called involution melancholia are in fact schizophrenic. To say that schizophrenic illness is more frequent in middle age than is usually admitted, or even that mixed schizo-affective psychoses are common, are logical statements, whether or not they are true. But to discuss vaguely transitions or part transitions from affective to schizophrenic psychoses makes diagnostic discussion difficult, unless one pursues such implications to their logical end, namely, a further evaluation of the 19th century concept of the *Einheitspsychose*, recently discussed by Cobb (1943), and in the specific field of depression by Good (1946).

Further possible sources of contamination, likely to affect the frequency of such symptoms as are mentioned above, lie in the inclusion in case material of more elderly patients, with arteriosclerotic or senile manifestations, a point taken as regards prognosis by Dreyfus in 1907. More recent surveys, such as that by Anderson already quoted, or that of Malamud, Sands and Malamud (1941), have excluded patients with any evidence of organic defect, but the earlier material still influences some accounts of involutional depression.

4. The prognosis in involutional melancholia has formerly been regarded as in general poor. Those authors, however, who report the results of convulsive therapy (e.g. Huston and Locher, 1948) give ample warrant to Sargant and Slater's statement that "of all depressive syndromes those of later life react best", and indeed this is likely to be the opinion of any experienced clinician. The reasons for this reversal, which was mentioned in its relation to duration in the introduction to this paper, are far from clear. It seems likely, however, that some, though by no means all, of the previous opinions about prognosis were affected by the inclusion of organic psychoses. Possibly some of the more chronic pictures were also the result of hospitalization, and of lack of attention to patients whose age made them physically less active and mentally less adaptable. Thus the reasons for the relative absence of such states at the present time may be in part similar to those responsible for the present infrequency of chronic catatonic syndromes in schizophrenia.

5. The pre-morbid personality of those individuals who develop involutional melancholia is said to be typical, or, if not so, at any rate likely to be different from the personality structure of patients with manic-depressive depressions. Most popular is an emphasis on a meticulous, rigid, inhibited, or obsessional temperament, which was stressed by Titley (1936) and by Palmer and his co-workers (Palmer and Sherman, 1938; Palmer, Hastings and Sherman,

1941). Anderson delimited a further category of the over-anxious, timid and sensitive, and found them as frequent as the obsessional. Malamud, Sands and Malamud described several different personality groupings, whose differentiation is not easy to follow; but their general conclusion was of a frequency of types of personality organization lacking in plasticity and restricted in interests. Most recent but not all earlier authors tend to find cyclothymic temperaments rare, but sometimes this in itself is used as the only point of diagnostic exclusion, which seems methodologically a little unfair. Of all such comments, one can only say that the problems of retrospective assessment of personality are enormous. Where only descriptive terms are used, where no theory of personality structure underlies the assessments, where no standard test is exhibited which might be repeated, it becomes difficult to communicate or compare the findings of different authors, and the possibility of an unconsciously selective bias in the gathering of historical evidence is always present.

6. It is said, lastly, that some aetiological factors have a specific relation to involutional melancholia.

Of its more distant origin, some have regarded it as genetically distinct from manic-depressive psychosis, others have not. It has been reported as genetically allied to schizophrenia, which may conceivably in part reflect problems in the selection of case material; for here, even more than in other psychiatric conditions, genetic studies are haunted by varying standards of diagnosis.

More immediate precipitating factors have been described, ranging from such stresses as physical illness (which was particularly obvious for the genito-urinary system in Malamud's material) to financial loss, employment difficulties, and so on, but the question of how far such strains are peculiar to the involutional melancholic has justly been raised. It is sometimes stated that environmental precipitation is much more evident in the involutional illness than in depressions occurring earlier in life. But this assertion perhaps accords ill with the equally common statement that involutional depressions are slow to flower, in comparison with earlier ones.

More closely specific are aetiological ideas relating firstly to the menopause itself, and secondly to a notion of the whole involutional epoch as a time of biological regression or dissatisfaction, or alternatively or additionally as a preparation for the senium.

Most psychiatric writers tend to reject the immediacy of the physical menopause as a frequent or sufficient cause; thus both Anderson and Palmer find it rarely significant. Farrar and Franks (1931) express a minority view in this matter, and their data show that in 60 per cent. of their cases first evidences both of menopause and psychosis arose within the same year. Malamud in his original paper thought that where it was closely coincidental this might be of good prognosis, but the point is not pursued in the long-term follow-up study of his material (Malamud, Sands, Malamud and Powers, 1949). One modern writer (Malleson, 1953) who was primarily concerned with the menopausal syndrome, believed that this may shade almost by a quantitative increase into, if not an involutional depression, at any rate an E.C.T. reactive state. Benedek (1950) viewed the climacteric syndrome as separate from psychotic depressive illness, but like Malleson regarded hypo-ovarianism as activating an illness, which she in turn described as "reactive depression—the characteristic psychiatric picture of the climacteric woman". More elaborate endocrine changes were postulated by Hemphill and Reiss (1940) who thought that four endocrine patterns were to be observed. It is likely, again, that different

selections of case material have produced these varying views on the importance of endocrine changes. Most authors are however agreed that developed psychotic pictures are not responsive to hormone therapy.

The concept of a more general epoch of involutinal strain is a wide one, as expressed by Henderson's pithy remark that the mind is occupied by the "might have beens"; or in greater detail by the more poetic statement of Curran and Partridge (1955): "The patient has realized his or her assets and has made of them what can be made: the interesting if illusory potentialities that may be lying round each corner of the future of youth are seen no longer to exist: the future is now inescapable and is what the patient's past has caused it to be. The bed is made and must be lain on. It is easy for middle-aged people to view the past with remorse and the future with dismay. Involutinal melancholics do so." In agreement with such global comments some authors feel that adequacy of sexual or family life, or of good early object relationships, are of good prognosis for the melancholic; but these assertions do not easily answer the question of why, therefore, symptoms arise in the first place. The general theme of biological regret is emotionally appealing but intellectually lacking in clarity; above all lacking in a clear definition in time, for involutinal psychoses are described from the later thirties to the later sixties.

It is natural in this connection to wonder what light psycho-analytic thought can throw on the psychological aspects of biological regression. Recent publications from Glasgow (Freeman and Cameron, 1953; Cameron, Freeman and Stewart, 1954; Cameron and Freeman, 1955) have reported the study and group psychotherapy of patients with involutinal depression who had not reacted well to E.C.T. The dynamics evident in this highly selected group are most profitably discussed, but largely—at least to the non-analytic reader—in terms based on orthodox Freudian views of depression in general, without great reference to uniquely involutinal events. Benedek, in the paper already quoted, and Fessler (1950) pay more attention to specific psychological problems of the climacterium, but both authors exclude from their consideration clearly psychotic states, Fessler openly describing these as endogenous depressions. Nevertheless, as we are here concerned with those elements in the broad involutinal picture which might be age-specific, we may legitimately mention their views.

Benedek feels that many psychological aspects of the climacterium seem to represent "a repetition of puberty, especially the pubertal reaction to menstruation", and finds that "the persistence of premenstrual symptoms, their severity and character, have a prognostic significance for the climacterium"; that is, because of the now permanent decline in hormonal production. The lack of hormone diminishes the ego's integrative strength, and this in turn combined with the inevitable desexualization of emotional needs requires a process of sublimation and further integration. The level of libidinal development formerly achieved affects regressive capacity, and hence the appearance and persistence of symptoms. Fessler thought that climacteric depression had "a marked resemblance to hysteria", and found the defence mechanisms of conversion, phobia and the hysterical character preceding climacteric depression in many of his cases. The menopause is taken as provoking regressive processes, and reviving the original frustration of penis lack, which had been overcome by the child-bearing capacity now lost. In the end Fessler adds two more to our gallery of personality types. His are the over-frustrated, chronically disappointed, belittling individuals, who had self-depreciatingly rejected their genital endowment, and the masculine, intolerant, aggressive, who had characteristically denied any anatomical deficiency.

METHOD OF THE PRESENT INVESTIGATION

The immediate impetus towards a clinical re-investigation of a group of involutional illnesses came from a proposal to collect, with a view to testing other hypotheses, a set of electroencephalographic records from patients in this age group. The EEG side of the enquiry will not be referred to further in the present paper; but, as clinical data were in any event needed for purposes of correlation, it appeared to us opportune to acquire them in sufficient detail to make a general survey possible.

The history of such surveys reflects a gradual change in the nature of the material investigated. Earlier compilations included, as has been said, patients with organic complications, and with a history of previous attacks. Anderson carefully excluded the former group, and Malamud, Sands and Malamud the latter in addition, but their cases were still pre-selected as suffering from depressive psychosis. It seemed to us that because of difficulty in disentangling what attributes are of relevance specifically to melancholia, and what are only of general significance, it would be wiser to start with no pre-conceived hypotheses, and to include in our case selection middle-aged patients who were having their first concrete psychiatric illness, no matter what its nature was; hoping thereby ultimately to achieve some useful dichotomy between those who were psychotically depressed, and those who were not. With this broad inclusion, we agreed that patients should be described and measured first, and, for the purpose of this investigation, diagnosed afterwards.

The criteria for the selection of cases for clinical study were therefore outlined as follows:

1. The patients were women. There were two major reasons for this criterion: firstly, that the diagnosis of involutional melancholia is more frequently made in females (Bellak, 1952), and secondly that the use of female patients made possible a number of more concrete comments about sexual climacteric change.

2. Their age was between 40 and 55. This age grouping was chosen partly because it coincides with that which includes the common range of the physical menopause, and partly because, above the age of 55, factors of age tend increasingly to influence the results of the psychological testing which formed part of the investigation.

3. They were first admissions to any mental hospital, which for this purpose included psychiatric wards in a general hospital. This gave a crude measure of mental health in that previous social adjustment had been at least sufficient to avoid hospitalization, and also excluded the disturbing effects of treatment on various measurements.

4. As it was primarily desired to cover as wide a range of illness as possible, it was decided to include in this investigation only those patients admitted to the amenity section of our hospital. There it is probably true to say that a higher proportion of neurotic illnesses seek admission than do to free beds. At the same time, there is no converse exclusion of psychotic material, and disturbed individuals are as freely available as elsewhere. By this means we hoped to widen the diagnostic span of our sample without sacrificing a due representation of serious illness. This was effective, but at the recognized cost of having our sample biased towards higher occupational class, and to higher intelligence, although the latter trend was not statistically significant.

In conformity with these simple criteria, data were collected regarding 54 women consecutively entering two admission units in our hospital. The clinical data related to history, to current behaviour and symptomatology, and

to response to treatment. Psychological examination included the use of a Standard Psychological Interview, the Thematic Apperception Test, and the Rorschach Test. These were administered, scored, and compared by a colleague working quite independently of clinical information, and are separately reported (Orme, 1957).

The cases were treated and the original data gathered by two of the authors, and the computation and comparison of data handled by a third. After preliminary experiment, a schedule of the information desired was designed. With this in mind, a full but free history was taken and examination made, and only thereafter was this material checked against the schedule and any deficiencies repaired. By this technique the use of check-lists or questionnaires, so liable to diminish rapport and presumably accuracy, was avoided. Thereafter adequate notes were made of course, therapy and result. A similar although less closely structured technique was used in acquiring information about the patients' behaviour outside the medical therapeutic situation, that is in the ward, in recreational and occupational therapy, and in their relationship with other patients and staff. As we believe that rating sheets to be used by nurses are best limited to the most material items of behaviour—as, for example, in chronic dilapidated psychotics—the senior nursing staff were simply informed of our special interest in these patients, and asked to make the descriptive nursing notes which they normally compile as full as possible. From these spontaneous descriptions simple behavioural ratings were made.

There remained the question of whether any of the assessments, either in their original form or in the value assigned to them later, might be coloured by diagnostic pre-conceptions. The heterogeneous nature of the group guarded to some extent against the human tendency to approach one's patients with a set of diagnostic Gestalts and unconsciously bias the examination to suit. On the recording side, data from the case records were transferred to punched cards by the clinically independent author, in a horizontal rather than a vertical fashion; that is, punching any given item for a set of patients rather than punching a set of items for a single patient, a method which is the more tedious but which helps to diminish halo effects. Diagnosis was not entered on the cards until other data were complete, and the diagnosis was that made by the two therapists. The investigation was closed when a total of 54 women had been examined.

CLINICAL FINDINGS

The data available can be looked at in various ways. First of all, information about the whole population of 54 patients can be gathered, and compared with what similar information is available for the general population; but for many variables this control information is not readily obtainable, and we would very much wish to make a comparative study of presumptively normal involutional women. Apart from such general statements an internal comparison can be made in respect of certain sortings, as between depressions and those who are not depressed, hoping by this means to indicate factors of possible significance in involutional depression alone, regarding it as an entity. Alternatively we might, for example, sort our data in terms of quality of reaction to electroconvulsive or other therapy.

It is the former division which is discussed in the present paper. Twenty-nine of the 54 women were regarded diagnostically as having endogenous depressive psychotic illnesses; here no attempt was made at further subdivision, as between any who might appear traditionally "involutional", and

others. A further 20 suffered illnesses which were regarded as non-psychotic, including six hysterical reactions, six alcoholic patients, six anxiety states, and three women with reactive-depressive states. For reasons of convenience this miscellaneous non-psychotic group will henceforward be referred to as "neuroses". The remaining 5 cases were undoubtedly of late schizophrenic or paraphrenic nature. These will be left out of account in making our internal contrast, which will be between the 29 depressions and the 20 neuroses: none of the points to be stressed depend mathematically and improperly on their exclusion. Not all the data collected find a place in these comments, for in the end some historical items proved difficult to ascertain with any certainty, and some assessments proved too subjective to be of value.

The mean age of our 54 women was forty-seven, and the age of the depressive group did not differ from that of the neurotic. Their dates of admission fell between September, 1954 and February, 1956. In May, 1956, only 3 of the 54 remained in hospital. Four had been re-admitted and re-discharged, and a further 3 had had a very short period of absence before re-entry, which in their case was not counted as a separate admission. The mode of stay was under two months, and the mean stay a little longer: depressions did not differ from neuroses in this respect. The immediate prognosis for these women's illnesses was therefore good, and it was good for the depressions among them as a separate group. A six months' follow-up study is in progress, so far with reasonably gratifying reports.

We may next make some comments about the general background of these patients. The numbers are not large, and it is obviously sanguine to expect any single factor to emerge as of striking importance; but generalizations have been made, and have had some effect on psychiatric thought, on slender evidence, and it seemed worth while to look at the facts concerning our group.

Seven patients had close and clear evidence of family mental illness, which we defined in the first instance as having siblings or parents who had either been admitted to a mental hospital, or had had E.C.T., or had committed suicide. More distant, and also less tangible, evidence of psychiatric illness in relatives was also collected. The figures were small, insufficient to express any expectancy beyond the normal, and did not discriminate between the depressions and the neuroses. The ordinal place in the family group was noted. Traditionally precarious positions as only, first or last children, or as first girls, were not more frequent than might be expected, and these family places did not differentiate between depressions and neuroses. Of upbringing and possible deprivation, in 5 cases maternal death had occurred while the patient was under five years, and in a further 11 one parent had died before the patient achieved her majority. There is nothing unexpected in these figures, and the groups did not differ in this regard. Evidence of other types of deprivation and restriction was gathered, but its assessment and comparison proved unsatisfactory.

Concerning the biological history of our patients, the mean age at the onset of menstruation was 13·6 years, which compares with the normal average figure of 13·49, reported by Wilson and Sutherland (1953). Twenty-two patients had passed the menopause, 6 with artificial intervention. Fourteen of the 54 had had a gynaecological operation (this to include dilatation and curettage) at some time in their lives, a proportion which at first sight seems rather high, although we could not find a normal estimate for comparison. In none of these respects did the depressions significantly differ from the neuroses.

Of their marital state, 10 were single, 39 married, 4 widowed and 1 divorced. Age at marriage showed a normal distribution, as did fertility. Depressions did

not differ from neuroses in any of these ways. Evidence was gathered of sexual adjustment. Of 10 single women, 5 had never had any real male friendships, while the remainder had had at least one friendship sustained for some months; 9 of the 10 had no physical sexual experience. The sexual adaptation of the 44 married women was ultimately rated in three categories. Three women had a grossly negative sexual history, of either non-consummation or homosexual difficulty. Twenty had a neutral sexual history: having neither appetite nor satisfaction in the sexual act. Twenty-one came into the class of positive sexual response, for which our requirements were very far from stringent—it might be occasional or even rare. The implication of these figures is that our group may perhaps contain a higher proportion of sexually inactive women than might be expected. Comparative figures are difficult to find for this country: Slater and Woodside (1951) reported some orgasmic response, even if infrequent or insufficient, in 85 per cent. of their control group, which was drawn however from an urban working-class group of younger age than our series. Thirteen of the 29 depressions, and 5 of the 20 neuroses, were in our category of positive marital sexual response, but this difference is not significant.

Because of our doubts about the validity of retrospective assessments of personality, we included only one descriptive statement; that most commonly mentioned—of the rather rigid, meticulous, perhaps house-proud, particular but not pathologically obsessional character. Twenty-two women came into this category, 14 among the 29 depressions, and 6 among the 20 neuroses; the difference is not significant. A firm previous history of either mood swings, or of depressed states without treatment or hospitalization, was found in 11 patients, 7 in the depressive group and 4 in the neurotic; again the difference is immaterial.

In summary, then, we collected background information regarding attributes which have been reported or which might be thought to be of significance for the concept of involuntional melancholia, selecting data capable of reasonable accuracy in ascertainment and of some degree of quantification. Ignoring any possible dynamic interplay of experiences, there was little in the crude data indicating any distinguishing factors between our 54 patients and the normal expectations for women of their age. As between depressions and neuroses, no differences significant for the numbers available for study were found.

We shall now consider a set of items regarding events closely preceding illness or hospitalization. Fifteen of the patients, including 10 depressions and 4 neuroses, had had a relatively material physical illness within the two years before their admission to hospital—an illness of the magnitude of an operation, a physical trauma, or an entrance to a general hospital for treatment. Other stresses of various kinds were much more difficult to assess, and ultimately only those happenings which seemed immediately related in time to the onset of symptoms were considered. In only 3 of the 54 cases was a major real bereavement associated sharply with the onset of illness. Miscellaneous stresses, of the order of loss of work, disruption of home, severe marital disagreement, etc., and here including physical illnesses, were apparent at the onset of illness in 21 cases, including 10 depressions and 8 neuroses. The meaning of these findings is hazardous to estimate, because of the absence of any control series and because also of the possibility of incipient psychiatric illness influencing events.

As a final specific happening we considered either the actual occurrence of the physical menopause, or the clear appearance of such premonitory symptoms as menstrual irregularity or marked flushings, where these events were immediately linked in time with the onset of symptoms. This was the case

in 17 of the 54 patients; 12 of them were depressed and 5 neurotic, a bias which is not significant. As has been mentioned earlier, most though not all psychiatric writers tend to discount the direct influence of the menopause on involutional illness. One wonders whether, in some cases, this factor has not been obscured by the relative chronicity of the case material before it came under observation.

The duration of the illness, before the patient was admitted to hospital, was measured. Of the 54 patients, 7 had been ill for less than three months, and 22 more for less than one year. Twenty of the 29 depressions were ill for under a year, and this contrasted significantly with the 20 neuroses, in only 4 of whom were symptoms of such short duration. (χ^2 (Yates)=6.8: $P < .01$.) The onset of depression therefore did not appear to be unduly insidious, as it is often said to be in this age range.

The last heading of the clinical findings relates to symptomatology. It must first be said boldly that the typical features of traditional or developed involutional melancholia were remarkable in this material only for their almost complete absence. Bizarre hypochondriasis, nihilistic delusions, great agitation, and so on, were confined to a handful of cases, and often occurred there only in isolation or for a very short period. Of the recorded items we can make the following comments.

A number of symptoms did not differentiate between our depressed and neurotic patients. For example, an emphasized complaint of exhaustion or fatigue occurred in 29 patients, without discrimination between the two groups. Apprehension and tension, not amounting to sustained fear or motor agitation, was likewise proportionately shared among a total of 33 patients. Loss of appetite, and/or loss of weight, were common (34 patients); loss of weight was more evident in the depressed (13 of 29) than the neurotic (6 of 20), but not significantly so. Hypochondriasis was divided into two grades. The lesser, which we called "repetitive hypochondriasis", included only complaints which were expressed in reasonably meaningful physical terms—as of headache, rheumatism, palpitation, etc. These, among 22 patients, were voiced by 12 depressions and 10 neuroses. Only 5 of the depressions, and no neuroses, achieved our second category of "psychotic hypochondriasis"—of any even mildly bizarre complaint. Thus the remaining 12 depressions expressed no hypochondriacal concerns at all. Obsessive-ruminative trends were seen in 3 depressions, and 3 neuroses. Simple insight did not materially differ in our groups; only 6 of the 54 patients (3 of these being schizophrenic) were utterly without realization that their illness might legitimately be regarded as psychiatric in nature.

Those symptoms which did seem to discriminate significantly between the two groups were limited to the constellation of pure depression, and to a lesser extent to secondary psychotic manifestations. Thus 52 of the 54 patients offered some complaint, however transient, of being depressed. However, a depression rated as acute, evident, sustained, and effectively unresponsive to the environment occurred in 20 of our depressive group, and in none of the neurotic. ($\chi^2=20.5$: $P < .001$.) Only 7 of the 54 patients did not have some complaint of sleeplessness; but late insomnia, of the early morning type, was found in 20 depressions and only 3 neuroses ($\chi^2=11.6$: $P < .001$). Diurnal variation of symptoms was recorded in 12 depressions, and in only 1 neurosis ($\chi^2=6.2$: $P < .02$). Self-reproach of psychotic and unreal quality was found in only 7 depressions, but a lesser degree of what was called self-criticism in a further 11; only 4 neuroses were self-critical ($\chi^2=6.8$: $P < .01$). Objective or subjective retardation of some extent was noted of 16 patients in the depressive group,

and 4 in the neurotic ($\chi^2=4.7$: $P<.05$). Motor agitation, which was recorded even if only of small extent, was seen in 8 of the 29 depressions, and none of the neuroses. Of the occurrence of these two latter symptoms in the depressive group, we may therefore say that neither was agitation very frequent nor retardation unusual. Of secondary psychotic symptoms among the 29 depressions only 6 were positively deluded, and only 2 actively hallucinated; and clearly paranoid ideation was recorded only in 4. One neurotic patient claimed hallucinatory experience. Regarding the various descriptive assessments of ward attitudes, in respect of demanding behaviour, dissatisfaction, early and late contact with fellow-patients, etc., only in one respect was there an indication of difference between the groups. Two of the depressive patients were assessed as unduly dependent in their current behaviour, while 8 neurotics were so regarded.

Summarizing the symptomatology, therefore, we found that the 29 depressions among our 54 patients showed effectively only the primary signs of a psychotic depression, and rarely any of the stigmata which are or were ordinarily regarded as peculiarly typical of involutional melancholia. It was only in respect of these primary signs, which are regarded as non-specific for age, that their symptoms differed significantly from those of 20 patients suffering from neuroses.

DISCUSSION

The findings of the clinical side of the present investigation are essentially negative in a number of different ways. A simple survey of certain items in the past history of women undergoing their first major psychiatric illness in the involutional period failed to show any material peculiarities which might easily distinguish them from what we know or guess about normal women in these respects. Of the 54 women, 29 showed to our minds a picture of psychotic depression; but these depressions did not seem markedly distinct, symptomatically, from psychotic depressions appearing at other ages, and it was only as regards their purely depressive aspects that they differed from 20 neuroses in the group. Background data, and assumed precipitating factors, similarly did not discriminate between the two sub-groups.

The data we were considering were crude, and the question of any dynamic interplay of factors has not here received attention. However, it is in respect of such single descriptive items that involutional melancholia achieves its usual recognition. Analysis of the psychological test data (Orme, 1957) may indeed indicate that the women in this series were more than ordinarily concerned about certain psychodynamic constellations, but this dimension of pre-occupation was not primarily related to whether or not they suffered from a depressive psychosis. If, for example, one arranges all these women in rank order of their typicality in respect of the test responses, those who were clinically specified as being depressed do not significantly cluster together on such a scale. We may therefore entertain the notion that there is a dimension of being psychiatrically ill during the involutional period, but that this may be separate from the dimension of being psychotically depressed. In more old-fashioned terms, any contribution of the involutional period to the clinical picture of depression may be accidental or at best pathoplastic, and the origin of the depression itself therefore remains just as autonomous as that of other endogenous psychoses.

For our small sample, then, we found that our depressions were not symptomatically of the classical "involutional" mould, and they were not

aetiologically isolated from non-depressive illnesses appearing at the same biological period. Reasons for our negative findings require some consideration. It might be that our method of selection failed somehow to include a due proportion of serious or malignant illnesses; but the cases were consecutive, the mental state of the patients often alarming, and while our hospital receives a number of patients from outside its immediate region the reason for such referrals often is related to severity rather than otherwise of the particular syndrome. Nevertheless, it might be argued that an actively therapeutic hospital, with a high proportion of voluntary admissions, inevitably receives many early cases, where the traditional symptomatology might be embryonic, and where its further development is inhibited by treatment. This view of a modern attenuation of the picture of involutional melancholia may well be true. But inevitably its acceptance then raises the further question of what symptoms are essential, and what are to be regarded merely as secondary elaborations, or chance accretions in a chronically hospitalized patient.

The survival value of any descriptive syndrome may be a just criterion of its reality, and despite the historical vagaries of the concept of involutional melancholia it retains a place in current nomenclature. The question nevertheless arises whether involutional melancholia remains a useful working classification in clinical psychiatry, or whether the retention of the word "involutional" as a qualifying adjective of particular meaning for depressive psychoses does not obscure rather than clarify our thinking about affective illness. Such a question has been put often in the past, and all the present report claims is that perhaps today it may be asked more pointedly and urgently.

SUMMARY

1. The present status and something of the origins of the diagnostic concept of "involutional melancholia" are discussed. Reasons are advanced for believing the boundaries of this traditional syndrome to be more nebulous than those of other diagnostic groupings.

2. A clinical survey of 54 consecutive female first admissions, between the ages of 40 and 55, is reported. Analysis of historical and symptomatic observations shows a remarkable absence of traditional pictures. Internal comparison of those patients who suffered from a psychotic depression, and those who did not, betrayed no obvious differences between the groups, unless in regard to the ordinary primary symptoms of endogenous depression. The meaning of these findings is discussed.

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A SURVEY OF CHRONIC PATIENTS IN A MENTAL HOSPITAL

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INTRODUCTION

A STATISTICAL study of large numbers of chronic mental patients may seem to have no direct bearing on the problem of how to treat the individual patient and prevent chronicity. Yet the collection and analysis of mass data form an essential part of epidemiological studies which have made possible many notable advances in preventive medicine. Much of the statistical information at present available in various official documents such as the annual reports of the Ministry of Health and the Board of Control, the Registrar-General's Statistical Review, are so generalized and fragmentary as to be almost meaningless (Lancet, 1955). A few mental hospitals publish annual reports containing broad statistical information; but the detailed facts needed for proper assessment of the size and nature of the problem and for any rational reform are almost invariably lacking. Throughout the field of psychiatry, particularly in connection with the development and effect of chronicity, the existing vital statistics are incomplete and inconclusive.

Over 40 per cent. of the hospital beds in the United Kingdom are occupied by the mentally ill and a high proportion of these are chronic in terms of length of stay in hospital. A single chronic mental patient may occupy a bed that in a general hospital might serve over a hundred patients during an equivalent period. Many different solutions to the problem of the chronic mental patient have been proposed or adopted, but few have been preceded by adequate ascertainment of the facts.

It seemed, therefore, worth while to carry out a detailed survey of the chronic population of a single mental hospital within a limited period (January to March, 1955). One could hope to get some data indicating the most profitable lines on which to attack the problem of overcrowding which is especially marked in the hospital concerned. It was clear that the patients to be studied were not a representative sample of the total chronic mental hospital population of Great Britain, but one could hope that the data obtained would be of interest to other psychiatrists faced with similar problems and would perhaps stimulate similar surveys elsewhere.

Methodology

The hospital is situated in a closely built-up area near the centre of a large industrial city. It was built over 100 years ago and in spite of modifications remains ill-adapted to modern views on the rehabilitation of long stay patients. In addition to the main block there are three annexes for chronic patients;

one for females situated within the curtilage of the main hospital and two for males placed some six miles away. If overcrowding indices are ignored the whole hospital can accommodate some 1,400 patients. Wards accommodating chronic patients vary in size from 20–112 patients.

For the purposes of the enquiry chronic patients were defined as patients who had been in hospital continuously for two or more years at the time of the enquiry. The period of two years was selected as the most suitable dividing line since experience has shown that the chances of discharge fall rapidly after two years' continuous hospitalization. By such a definition some patients may be omitted who have been in hospital for less than two years but are nevertheless bound to become chronic patients. It also misses a number of patients who through the application of modern physical treatments, are discharged and re-admitted repeatedly, thus masking their chronicity by short returns to their families. On the other hand some patients are included who may recover and be discharged after two years in hospital. Nevertheless other criteria, clinical or diagnostic, of chronicity are open to objection on other grounds and lack the simplicity of the definition adopted.

A *proforma* was designed on which the required information about each patient could be rapidly recorded and later transcribed on to punched cards for statistical analysis. Draft *proformas* were subjected to a pilot survey and modified accordingly. As originally planned the survey was to take place in two parts; part 1 was to be concerned mainly with data concerning the patient in hospital, and part 2 concerned with the patients' current family and social background. Owing to the lack of a psychiatric social worker, part 2, which was to be carried out in a field survey had to be abandoned.

While Part I of the *proforma* (see Appendix) was based on the case records and administrative records of the hospital, Part II dealing with the patient's behaviour, employment and nursing requirements was ascertained by interviewing the nursing staff and by examination of nurses' notes. The various categories are self-explanatory—except perhaps for 20–23 dealing with the patients' abnormal behaviour. Here special care was taken to explain the meaning of the terms to the nurses. In cases where clinical data were in doubt the patient was seen and examined. Care was taken to ensure that information about individual patients was obtained from nurses who knew the patients well by their long contact with them; in some wards this necessitated interviewing the deputy sister or charge nurse.

The information of Part III of the *proforma* was obtained from the patients' visiting and parole cards. The former are kept at the Porter's Lodge and each visit to the patient is recorded.

Some errors in assessing a number of various factors are probably inevitable in a large scale survey of this type. Nevertheless it was felt that recording and rating errors were not so large as to have a significant influence on the results.

FINDINGS

Age and Sex Distributions

There were 442 male and 654 female patients who had been in residence at the hospital for over two years at the time of the enquiry. The distribution of these patients by age, sex and length of stay is shown in Table I, using 10-year age-groups and 5-year groups for length of stay. Whereas slightly more than half the male patients are aged between 40 and 59 years, almost half the female patients are aged between 50 and 69. Differences between the two percentage age distributions are apparent from Figure 1.

TABLE I
Distribution of Patients According to Age, Sex and Length of Stay

		(a) Males							
Age Group		Duration of Stay (Years)							
		2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
<30	..	8	6	2	—	—	—	—	16
30-39	..	10	26	14	8	—	—	—	58
40-49	..	15	25	33	21	11	6	—	111
50-59	..	8	14	12	37	20	19	12	122
60-69	..	5	11	3	7	15	11	18	70
70-79	..	5	11	3	12	8	5	15	59
80+	..	1	1	—	1	1	1	1	6
All ages	..	52	94	67	86	55	42	46	442

		(b) Females							
Age Group		Duration of Stay (Years)							
		2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
<30	..	6	4	—	—	—	—	—	10
30-39	..	8	13	12	12	1	—	—	46
40-49	..	9	29	29	30	19	2	2	120
50-59	..	10	20	26	29	34	27	9	155
60-69	..	13	23	16	34	37	26	22	171
70-79	..	15	13	16	17	21	8	16	106
80+	..	5	2	2	11	11	2	13	46
All ages	..	66	104	101	133	123	65	62	654

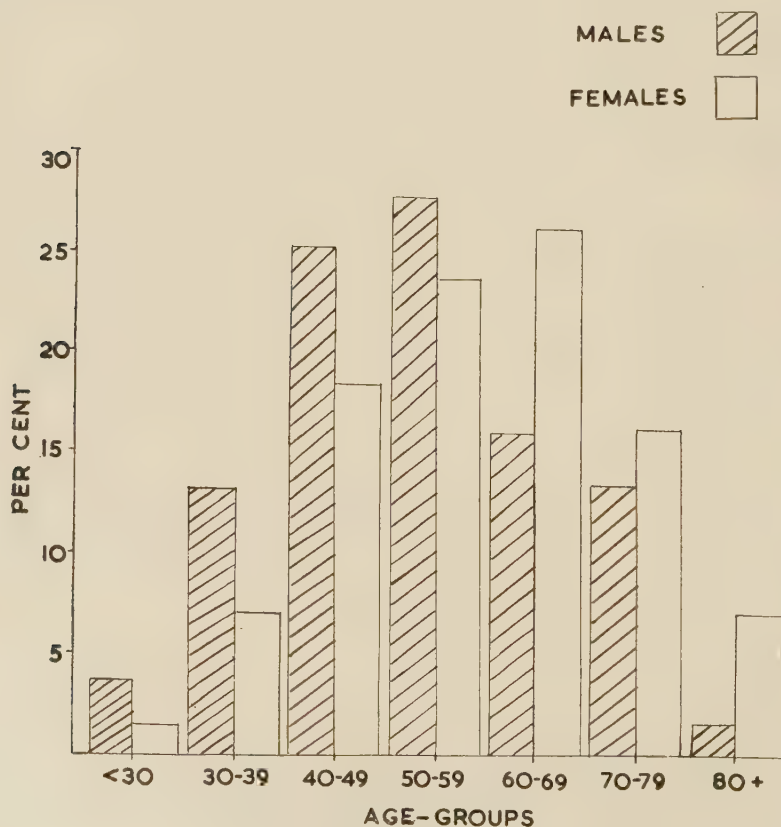


FIG. 1.—Percentage age distribution of all patients.

Length of Stay

Examination of length of stay distributions shows that of male patients, 66 per cent. had been in residence for more than ten years; the corresponding female proportion was 74 per cent. Of male patients aged 60 years or over, 75 per cent. had been in residence for more than 10 years and 55 per cent. for more than 20 years. Corresponding proportions for females in this age-group were 79 per cent. and 48 per cent.

Re-admissions

The number of previous admissions was recorded for each patient and 57 per cent. of male and 61 per cent. of female patients had become chronic patients after their first admission. On relating length of stay to the number of previous admissions it was found that of patients who had been in hospital for more than ten years, 65 per cent. had been admitted only once, whereas the corresponding figure for patients who had been in hospital for less than ten years was 47 per cent. (Table II).

TABLE II
Duration of Stay (Years) and Number of Previous Admissions
(Percentage distribution for each group)

Males								
No. of Previous Admissions	2-4	5-9	10-14	Duration of Stay (Years)				
	15-19	20-24	25-29	30+	Total			
0	36.5	43.6	50.8	70.9	65.5	64.3	78.2	57.4
1	32.7	27.7	28.3	22.0	23.7	28.5	15.2	25.6
2	17.3	19.1	16.4	3.5	7.2	—	2.2	10.4
3 or more	13.5	8.5	4.4	2.4	3.6	—	2.2	5.2
Not known	—	1.1	—	1.2	—	7.2	2.2	1.4
All patients	100.0 (52)	100.0 (94)	100.0 (67)	100.0 (86)	100.0 (55)	100.0 (42)	100.0 (46)	100.0 (442)

Females								
No. of Previous Admissions	2-4	5-9	10-14	Duration of Stay (Years)				
	15-19	20-24	25-29	30+	Total			
0	54.6	50.9	61.4	63.1	62.6	66.2	72.6	61.2
1	28.8	32.7	26.8	25.6	25.2	24.6	16.1	26.1
2	12.1	9.7	8.9	7.5	7.3	3.0	4.8	7.8
3 or more	3.0	4.8	2.9	2.3	3.2	3.2	3.2	3.4
Not known	1.5	1.9	—	1.5	1.7	3.0	3.3	1.5
All patients	100.0 (66)	100.0 (104)	100.0 (101)	100.0 (133)	100.0 (123)	100.0 (65)	100.0 (62)	100.0 (654)

Re-admission rates were also calculated for each age-group (Table III). Those referable to male patients under 50 years of age were slightly higher than those for subsequent age-groups. The female distributions present a more striking pattern. The proportion of patients with no previous admission

TABLE III
Age and Number of Previous Admissions
 (Percentage distribution for each age-group)

No. of Previous Admissions	Males						All Ages
	<40	40-49	Age-Group (Years)		70+		
			50-59	60-69			
0	54.1	42.3	66.4	68.6	58.5		57.4
1	27.0	32.5	19.6	22.8	26.2		25.6
2	12.2	16.2	10.7	4.3	4.6		10.4
3 or more ..	5.4	9.0	1.6	2.8	7.7		5.2
Not known ..	1.3	—	1.6	1.5	3.0		1.4
All patients ..	100.0 (74)	100.0 (111)	100.0 (122)	100.0 (70)	100.0 (65)		100.0 (442)

No. of Previous Admissions	Females						All Ages
	<40	40-49	Age-Group (Years)		70+		
			50-59	60-69			
0	41.1	54.1	59.4	65.5	71.1		61.2
1	39.3	35.0	27.0	22.2	17.8		26.1
2	17.9	7.5	9.0	6.5	4.6		7.8
3 or more ..	—	2.5	3.9	3.9	3.9		3.4
Not known ..	1.8	0.8	0.7	1.8	2.6		1.5
All patients ..	100.0 (56)	100.0 (120)	100.0 (155)	100.0 (171)	100.0 (152)		100.0 (654)

increased consistently with age; the converse was true of patients with one previous admission. Of female patients under 40 years of age, 18 per cent. had been admitted twice previously.

Abnormal Behaviour

It was hoped to obtain a behaviour profile of each patient by recourse to the list of emotional states shown on the proforma. In actual practice it was found that many patients could not be clearly rated by the relatively simple scale used. The analysis was therefore confined to identifying those exhibiting offensive tendencies, i.e. those patients who had one or more of the following states: suicidal, aggressive, destructive, restless, irritable or were continually undressing. It was found that 51 per cent. of male and 47 per cent. of female patients fell into this group.

Presence of Physical Diseases

The prevalence of physical disorders was an important feature of this enquiry and is further considered in relation to mobility of patients. Overall proportions for males and females were 28 and 33 per cent. respectively and Table IV gives such proportions by age-group. These increase with age; the

TABLE IV
Patients with Physical Diseases by Sex and Age-Group

				Sex		
				Males	Females	
Age-Group (Years)			No.	Proportion of All Patients	No.	Proportion of All Patients
Under 40	..	..	11	14.9	12	21.4
40-49	..	..	22	19.8	20	16.7
50-59	..	..	27	22.1	39	25.2
60-69	..	..	25	35.7	59	34.5
70 and over	..	..	40	61.5	88	57.9
All ages	..	..	125	28.3	218	33.3

figure for patients aged 70 years and over is outstandingly high. The relatively high proportion for females under 40 years of age is due to the provision of a female ward for patients with pulmonary tuberculosis.

Conditions which are included in these totals, grouped according to the International Statistical Classification of Diseases, are given in Table V for males and females separately. Some patients have more than one of the

TABLE V
Physical Diseases Present

Diagnostic Category (I.S.C.)								Males	Females
Diseases of Circulatory System								36	90
Hypertension								10	50
Diseases of heart								19	17
Arteriosclerosis								2	17
Other								5	6
Diseases of Digestive System								14	1
Hernia								13	1
Other								1	—
Eye Diseases								11	11
Blindness								7	9
Other								4	2
Neoplasms								2	10
Diseases of Ear and Mastoid Process								11	20
Deafness								11	19
Other								—	1
Diseases of C.N.S.								8	17
Diseases of Bone and Organs of Movement								8	7
Arthritis								3	3
Other								5	4
Injuries								5	9
Diseases of Skin and Cellular Tissue								8	7
Respiratory Diseases								7	26
Bronchitis								7	23
Other								—	3
Diabetes								1	6
Anaemia								1	5
Pulmonary Tuberculosis								—	14
Other								22	30
Total								134	253

categories shown and in such cases are included more than once. Hypertension and heart diseases together account for more than one-fifth of all physical diseases of male patients, and hypertension alone accounts for the same proportion for females; bronchitis contributes 9 per cent. and pulmonary tuberculosis 6 per cent. to the female total.

Employment of Patients

Table VI shows that 40 per cent. of male patients and 54 per cent. of female patients were entirely unemployed. Those shown as working on the ward are mainly engaged on domestic duties and such employment is likely to be nominal and sporadic. A higher proportion of males than females were working elsewhere in the hospital, although the actual numbers were almost identical. Female patients worked mainly in the sewing room, kitchens and laundry, whereas male patients were engaged in the kitchens, on the farm and gardens and on maintenance work. No patients were working outside the hospital grounds. As was to be expected, the proportion of patients unemployed tends

TABLE VI
Employment of Patients by Age
 (Percentage distribution for each age-group)

Males						
Place of Employment	Age-Group					All Ages
	<40	40-49	50-59	60-69	70+	
Not working	33.8	32.5	42.6	42.9	50.8	39.8
On ward only	45.9	41.4	35.2	38.6	26.2	37.8
On ward and elsewhere ..	—	3.6	1.7	1.4	1.5	1.8
Elsewhere in hospital ..	20.3	22.5	20.5	17.1	21.5	20.6
All patients	100.0 (74)	100.0 (111)	100.0 (122)	100.0 (70)	100.0 (65)	100.0 (442)

Females						
Place of Employment	Age-Group					All Ages
	<40	40-49	50-59	60-69	70+	
Not working	53.6	49.1	51.6	52.0	63.2	54.1
On ward only	35.7	37.5	32.9	30.4	27.6	32.1
On ward and elsewhere ..	—	—	0.6	0.6	—	0.3
Elsewhere in hospital ..	10.7	13.4	14.9	16.9	9.2	13.5
All patients	100.0 (56)	100.0 (120)	100.0 (155)	100.0 (171)	100.0 (152)	100.0 (654)

to increase with age. It was found that duration of stay had little effect upon the employment of patients.

Nursing Care Required

The four-fold classification of nursing care devised by Tait (1948) was used and results are shown in Table VII. The assessment of nursing care

TABLE VII
Nursing Care Required in Relation to Age of Patient
 (Percentage distribution for each age-group)

Males						
Nursing Care Required	Age-Group (Years)					All Ages
	<40	40-49	50-59	60-69	70+	
None	4.1	14.4	11.5	12.9	12.3	11.3
Routine	71.6	66.7	72.1	68.6	70.8	69.9
Routine plus special attention	18.9	16.2	14.8	15.7	12.3	15.6
More or less constant vigilance	5.4	—	1.6	1.4	4.6	2.3
Constant vigilance plus restriction from violence	—	2.7	—	1.4	—	0.9
All patients	100.0 (74)	100.0 (111)	100.0 (122)	100.0 (70)	100.0 (65)	100.0 (442)

Females						
Nursing Care Required	Age-Group (Years)					All Ages
	<40	40-49	50-59	60-69	70+	
None	5.4	9.2	20.0	24.0	25.7	19.1
Routine	62.5	67.5	62.6	60.8	61.2	62.7
Routine plus special attention	26.8	19.2	14.2	14.0	12.5	15.8
More or less constant vigilance	5.4	3.3	2.6	1.2	0.6	2.1
Constant vigilance plus restriction from violence	—	0.8	0.6	—	—	0.3
All patients	100.0 (56)	100.0 (120)	100.0 (155)	100.0 (171)	100.0 (152)	100.0 (654)

required by any group of patients necessarily involves a large subjective element and differing interpretations by ward sisters and charge nurses are likely to occur. This difficulty will chiefly influence the numbers stated as requiring no nursing care and those requiring routine care. Whereas the distinction between these two grades is liable to be subject to error, figures for the other categories will be more reliable.

Only very small proportions required constant vigilance, and about two-thirds of the patients required routine supervision. An examination of proportions for each age-group revealed that, although age does not seem to be an important feature, a greater proportion of patients under 40 years of age required special attention as compared with older age-groups.

Mobility of Patients

Table VIII shows that the large majority of patients (over 90 per cent.) are ambulant for the whole day. As might be expected the proportion of bed-

TABLE VIII
Mobility of Patients According to Age

Males						
Mobility	Age-Group (Years)					All Ages
	<40	40-49	50-59	60-69	70+	
Bed-ridden	3	1	1	3	10	18
Ambulant for part of day ..	—	3	3	2	2	10
Ambulant for whole day ..	71	107	118	65	53	414
All patients	74	111	122	70	65	442

Females						
Mobility	Age-Group (Years)					All Ages
	<40	40-49	50-59	60-69	70+	
Bed-ridden	3	4	9	7	18	41
Ambulant for part of day ..	2	2	3	1	6	14
Ambulant for whole day ..	51	114	143	163	128	599
All patients	56	120	155	171	152	654

ridden patients is highest in the older age-groups, and 56 per cent. of the male and 44 per cent. of female bed-ridden patients are over 70 years of age. The excess of female bed-ridden patients in the younger age-groups is partly accounted for by female patients with pulmonary tuberculosis.

About half the bed-ridden patients required only routine nursing care (Table IX). The high proportion of female bed-ridden patients requiring special

TABLE IX
Mobility of Patients and Nursing Care Required

					Sex and Mobility							
					Males			Females				
Nursing Care					Bed-ridden	Ambulant Part of Day	Ambulant Whole Day	Total	Bed-ridden	Ambulant Part of Day	Ambulant Whole Day	Total
None	..	..	..	..	—	—	50	50	—	1	124	125
Routine	..	..	..	..	10	6	293	309	19	8	383	410
Routine plus special attention	..	..	..	..	3	3	63	69	19	3	81	103
More or less constant vigil	..	..	..	..	3	1	6	10	2	1	11	14
Constant vigil	..	..	..	..	2	—	2	4	1	1	—	2
Totals	..	..	..	..	18	10	414	442	41	14	599	654

TABLE X
Degree of Supervision in Relation to Duration of Stay
 (Percentage distribution for each group)

		Duration of Stay (Years)							
		2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
Males									
Degree of Supervision									
Confined to ward entirely	..	32.7	28.7	22.4	23.3	30.9	28.6	30.4	27.6
Confined to ward except for messages, meals, etc.	..	1.9	1.0	1.4	—	—	—	—	0.7
Allowed outside ward under supervision	..	32.7	28.7	31.4	15.1	12.7	19.0	13.0	22.4
Allowed outside ward without supervision	..	32.7	41.6	44.8	61.6	56.4	52.4	56.6	49.3
All patients	..	100.0 (52)	100.0 (94)	100.0 (67)	100.0 (86)	100.0 (55)	100.0 (42)	100.0 (46)	100.0 (442)
Females									
Degree of Supervision									
Confined to ward entirely	..	34.8	30.8	25.7	23.3	17.8	27.7	34.0	26.5
Confined to ward except for messages, meals, etc.	..	—	0.9	1.0	0.8	0.8	1.5	—	0.8
Allowed outside ward under supervision	..	42.4	36.5	37.6	48.9	39.8	41.5	37.0	40.9
Allowed outside ward without supervision	..	22.8	31.8	35.7	27.0	41.6	29.3	29.0	31.8
All patients	..	100.0 (66)	100.0 (104)	100.0 (101)	100.0 (133)	100.0 (123)	100.0 (65)	100.0 (62)	100.0 (654)

nursing attention in addition to routine supervision is due to the patients with tuberculosis. Male and female proportions of ambulant patients requiring special attention and more or less constant supervision were almost identical. Differences between male and female proportions of patients requiring no care or routine care are probably artificial and arise because of different subjective interpretations of these classes of care.

Bed-ridden patients therefore need more nursing care than do ambulant patients and 72 per cent. of male and 93 per cent. of female bed-ridden patients had physical diseases as compared with only about one-quarter of ambulant patients.

Degree of Supervision

The four-fold classification used for determining the degree of supervision required is given in Table X. The proportions of males and females confined to the ward were almost the same. Almost half the male and one-third of female patients were allowed outside their wards *without* supervision. There is a complementary sex differential for patients allowed outside the ward *under* supervision. Length of stay in hospital appeared to have little bearing upon the extent of supervision required, although it will be noted that a relatively small proportion of patients who had been in residence for less than 5 years were allowed outside the ward without supervision.

Visiting of Patients by Relatives and Friends

The frequency with which each patient was visited and by whom, was recorded. Some patients were visited by more than one person, usually at different intervals. For the present purpose, the most frequent visit alone is included in the figures given below. As the extent to which patients are visited is likely to be associated with the degree of week-end parole, results are given in Table XI separately for patients who (i) did not go out on parole, (ii) went out on occasional parole and (iii) went out on regular parole. Whereas 9 per cent. of male patients were allowed occasional parole and 11 per cent. regular parole, the corresponding female proportions are only 5 per cent. and 4 per cent. respectively.

TABLE XI
Frequency of Visiting Related to Week-end Parole

Frequency of Visiting	Males				Females			
	None	Occasional	Regular	Total	None	Occasional	Regular	Total
Weekly	72	13	4	89	99	8	12	119
Monthly	103	12	7	122	141	13	2	156
3-monthly ..	35	3	4	42	78	4	1	83
Annually	13	—	2	15	38	3	1	40
Occasionally ..	46	3	7	56	65	3	4	72
Not at all ..	87	7	24	118	175	2	7	184
Total	356	38	48	442	596	31	27	654

Of all male patients, 27 per cent. were never visited and a further 13 per cent. only occasionally. Female proportions were almost the same, so that two out of every five chronic patients had no regular visitors. Roughly the same proportion were visited at either weekly or monthly intervals. Figures for patients who did not go out on parole closely resemble these. Of those patients on occasional parole, two-thirds had regular weekly or monthly visits. As was anticipated, a high proportion of male patients on regular parole were not

TABLE XII
Frequency of Visits by Duration of Stay

Males									
	Frequency	Duration of Stay (Years)							
		2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
Weekly ..	..	17	26	12	19	7	4	4	89
Monthly ..	..	13	27	23	18	18	11	12	122
3-monthly ..	..	2	11	5	8	9	4	3	42
Annually ..	..	1	2	2	4	2	1	3	15
Occasionally ..	..	10	12	8	15	2	4	5	56
Not at all ..	..	9	16	17	22	17	18	19	118
All patients ..	..	52	94	67	86	55	42	46	442
Females									
	Frequency	Duration of Stay (Years)							
		2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
Weekly ..	..	22	30	28	19	10	7	3	119
Monthly ..	..	16	34	24	31	26	14	11	156
3-monthly ..	..	10	10	10	17	19	8	9	83
Annually ..	..	4	7	5	7	4	5	8	40
Occasionally ..	..	7	6	7	17	19	7	9	72
Not at all ..	..	7	17	27	42	45	24	22	184
All patients ..	..	66	104	101	133	123	65	62	654

TABLE XIII
Frequency of Visiting by Age-Group

Males									
		Age-Group (Years)							
Frequency		<30	30-39	40-49	50-59	60-69	70-79	80+	Total
Weekly	..	..	18	21	22	12	8	1	89
Monthly	..	5	15	33	41	15	13	—	122
3-monthly	..	1	4	8	9	12	7	1	42
Annually	..	—	1	4	4	3	2	1	15
Occasionally	..	3	9	15	14	6	8	1	56
Not at all	..	—	11	30	32	22	21	2	118
All patients	..	16	58	111	122	70	59	6	442
Females									
		Age-Group (Years)							
Frequency		<30	30-39	40-49	50-59	60-69	70-79	80+	Total
Weekly	..	3	15	21	18	31	24	7	119
Monthly	..	2	13	33	31	37	27	13	156
3-monthly	..	2	4	16	24	25	10	2	83
Annually	..	1	3	5	14	10	5	2	40
Occasionally	..	1	2	12	10	27	16	4	72
Not at all	..	1	9	33	58	41	24	18	184
All patients	..	10	46	120	155	171	106	46	654

visited regularly; it is surprising that half the females on regular parole were visited at weekly or monthly intervals.

Frequency of visits was then considered in relation to length of stay and age of patient. Table XII shows that 39 per cent of male patients who had been in residence for 20 years or more received either weekly or monthly visits; the corresponding female proportion was 28 per cent. Of those patients who had been in residence for less than 10 years, almost 60 per cent. were regularly visited and the proportion falls off gradually as length of stay increases. A similar relationship exists between age and visiting (Table XIII). Whereas 50 per cent. of male and 41 per cent. of female patients aged 70 years or more were not regularly visited, the corresponding proportions of those under 40 years of age were 31 per cent. for males and 23 per cent. for females.

The principal diagnoses of chronic patients are given in Table XIV together with age and sex distributions. Schizophrenia accounted for 72 per cent. of male and 66 per cent. of female patients; affective disorders accounted

TABLE XIV
Diagnosis by Age

Diagnosis	Males							
	Age-Group (Years)							
	<30	30-39	40-49	50-59	60-69	70-79	80+	Total
Schizophrenic disorders	14	46	86	96	46	30	—	318
Manic-depressive reaction	—	1	5	4	5	7	1	23
Involuntal melancholia	—	—	—	3	3	6	1	13
Paranoia and paranoid states	—	—	—	1	—	1	—	2
Senile psychosis	—	—	—	—	—	4	2	6
Pre-senile psychosis	—	—	—	1	1	1	—	3
Psychosis with cerebral arteriosclerosis	—	—	—	1	4	3	1	9
Alcoholic psychosis	—	—	—	—	—	1	—	1
Psychosis of other demonstrable aetiology	—	6	9	11	2	3	—	31
Other psychoses	—	1	1	1	1	1	1	6
Psychoneurotic disorders	—	1	—	—	—	—	—	1
Pathological personality	—	1	—	—	1	—	—	2
Mental deficiency	2	2	9	4	7	2	—	26
Other	—	—	1	—	—	—	—	1
All patients	16	58	111	122	70	59	6	442
Diagnosis	Females							
	Age-Group (Years)							
	<30	30-39	40-49	50-59	60-69	70-79	80+	Total
Schizophrenic disorders	7	38	91	112	122	54	12	436
Manic-depressive reaction	—	1	10	10	7	11	6	45
Involuntal melancholia	—	—	2	8	12	10	3	35
Paranoia and paranoid states	—	—	1	—	1	—	—	2
Senile psychosis	—	—	—	—	2	21	16	39
Pre-senile psychosis	—	—	2	—	4	1	—	7
Psychosis with cerebral arteriosclerosis	—	—	—	—	2	2	2	6
Alcoholic psychosis	—	—	—	1	—	—	1	2
Psychosis of other demonstrable aetiology	1	2	7	5	6	—	1	22
Other psychoses	—	—	—	1	4	3	4	12
Psychoneurotic disorders	—	—	—	1	—	—	—	1
Pathological personality	1	—	1	—	—	—	—	2
Mental deficiency	1	4	4	13	9	4	—	35
Other	—	1	2	4	2	—	1	10
All patients	10	46	120	155	171	106	46	654

for 8 per cent. and 12 per cent. respectively, and 6 per cent. of male and 5 per cent. of female patients were diagnosed as mentally defective. The large sex differential in proportions of patients with senile psychoses reflects the differences between the age distributions, although it is likely that senile dementia in some cases was superimposed on previous schizophrenia. In what follows, those diagnoses which occur in sufficiently large numbers to merit further analysis are considered in relation to several aspects mentioned above.

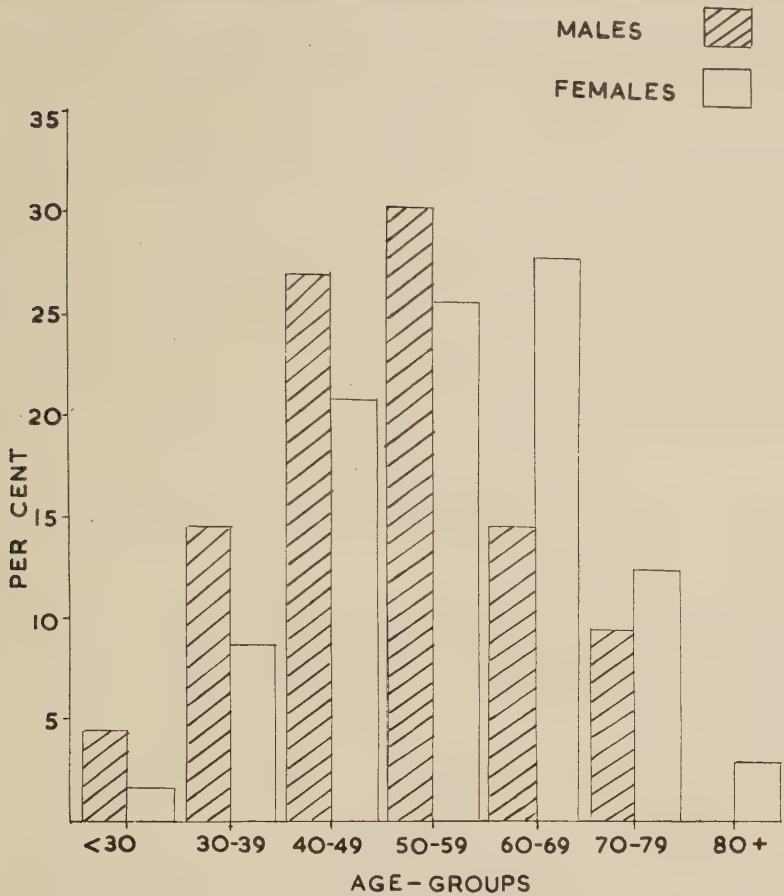


FIG. 2.—Percentage age distribution of schizophrenic patients.

Schizophrenic Disorders

Age distributions for this diagnostic category are depicted in Figure 2 which further emphasizes the sex differences. Table XV gives the results of relating age to length of stay. It will be noted that the large majority of patients had been in residence for 10 years or more; the female proportion exceeds the male. Furthermore, 70 per cent. of male patients aged 60 years or over had been in hospital for more than 20 years; the corresponding proportion for female patients in the age-group was 60 per cent.

Of the male schizophrenic patients, 21 per cent. had accompanying physical disorder of note and 8 per cent. had a mental disorder in addition to schizophrenia; proportions for female patients were roughly the same. For both sexes, over 40 per cent. of patients aged 60 years and over had physical diseases as compared with 15 per cent. of those under 60 years of age.

Table XVI shows the extent of nursing care required by schizophrenic patients. Of all male patients, 16 per cent. required more than routine supervision; the proportion for those under 40 years of age was 25 per cent. Figures for female patients were very similar and show that schizophrenic patients, especially those in the younger age-groups, make considerable demands upon trained nursing staffs.

TABLE XV

Distribution of Patients with Schizophrenic Disorders According to Age, Sex and Length of Stay

Males								
Duration of Stay (Years)								
Age-Group	2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
<30 ..	7	5	2	—	—	—	—	14
30-39 ..	6	21	11	8	—	—	—	46
40-49 ..	7	20	27	19	7	6	—	86
50-59 ..	4	11	9	29	16	18	9	96
60-69 ..	2	5	1	5	13	8	12	46
70-79 ..	—	3	2	4	4	4	13	30
80+ ..	—	—	—	—	—	—	—	—
All ages ..	26	65	52	65	40	36	34	318

Females								
Duration of Stay (Years)								
Age-Group	2-4	5-9	10-14	15-19	20-24	25-29	30+	Total
<30 ..	5	2	—	—	—	—	—	7
30-39 ..	4	10	11	12	1	—	—	38
40-49 ..	2	21	24	23	18	1	2	91
50-59 ..	2	13	16	20	31	22	8	112
60-69 ..	4	12	10	28	29	20	19	122
70-79 ..	2	4	4	10	18	7	9	54
80+ ..	—	—	—	2	7	—	3	12
All ages ..	19	62	65	95	104	50	41	436

Affective Disorders

Age and sex distributions are given separately for manic-depressive illness and involutional melancholia on Table XIV. For each there was an excess of female to male patients. A combination of these two diagnoses provides sufficient numbers to merit further analysis. Almost two-thirds of these patients were 60 years of age or over. Table XVII gives length of stay distributions and there are some differences between corresponding proportions for males and females. However, for both sexes more than one-third of the male patients and 37 per cent. of females had accompanying physical disorder of note; almost half the patients aged 60 years and over had such diseases. Few patients required more than routine supervision and male and female proportions were almost the same.

TABLE XVII

Duration of Stay Distributions for Certain Diagnosis

Diagnosis and Sex		Duration of Stay (Years)							Total
		2-4	5-9	10-14	15-19	20-24	25-29	30+	
Affective disorders	M	5	8	2	11	5	2	3	36
	F	14	16	17	16	6	5	6	80
Mental deficiency	M	3	7	1	2	5	3	5	26
	F	6	7	6	5	5	3	3	35
Epilepsy with psychosis	M	2	2	6	1	2	1	2	16
	F	2	1	2	4	2	2	—	13
Senile psychosis	M	3	2	—	—	1	—	—	6
	F	16	7	7	3	1	—	5	39

TABLE XVI
Nursing Care Required by Patients with Schizophrenic Disorders

		Males					
Care		Age-Group (Years)					
		<30	30-39	40-49	50-59	60-69	Total
None	..	—	1	11	12	5	35
Routine	..	11	33	60	74	34	232
Routine plus special attention	..	2	9	12	8	7	42
More or less constant vigilance	..	1	3	—	2	—	6
Constant vigilance plus restriction from violence	..	—	—	3	—	—	3
All patients	..	14	46	86	96	46	318

		Females					
Care		Age-Group (Years)					
		<30	30-39	40-49	50-59	60-69	Total
None	..	—	2	8	20	29	81
Routine	..	5	25	62	71	77	280
Routine plus special attention	..	2	8	16	19	14	63
More or less constant vigilance	..	—	3	4	1	2	10
Constant vigilance plus restriction from violence	..	—	—	1	1	—	2
All patients	..	7	38	91	112	122	436

Mental Deficiency

This condition was the principal diagnosis of 26 male and 35 female patients at the time of the survey, and it was a subsidiary diagnosis in the case of 18 male and 17 female patients. From the age distributions given on Table XIV it will be noted that two-thirds of such patients were under 60 years of age; about the same proportion had been in residence for 10 years or more (Table XVII). Of all male mental defectives, 50 per cent. had accompanying physical disorders of note and 58 per cent. had a secondary diagnosis of mental disorder. Corresponding female proportions were 34 per cent. and 49 per cent. Few patients required more than routine nursing supervision.

Epilepsy with Psychosis

An age distribution is given for each sex in Table XIV, although it is necessary to combine results for the sexes to give sufficiently large numbers for further analysis. Nearly half the patients with this condition were under 50 years of age. Table XVII shows that almost one-third had been in residence for 20 years or more. With only two exceptions, these patients were ambulant for the whole day and 62 per cent. were working on the wards; the remainder were unemployed. Four patients of each sex had some physical disorder of note and five males and one female had a subsidiary mental disorder. Almost one-third of these patients required more than routine nursing care, although only one needed more or less constant vigilance.

Senile Psychosis

Mention has already been made of the large excess of female patients with such conditions and the following comments refer to all patients. Only two patients were under 70 years of age and 40 per cent. were 80 years of age or over (Table XIV). Almost the same proportion had been in residence for less than 5 years (Table XVII). As was to be expected, a high incidence of physical diseases was recorded (60 per cent.) and 10 patients (23 per cent.) were confined to bed. Almost one-third of these patients required special attention in addition to routine nursing care although none needed constant vigilance.

DISCUSSION

From the total findings of our survey, this paper only presents a selection of facts from which practical conclusions may be drawn. Material, for instance, relating to small diagnostic groups, or data referring to the areas of the city in which the patients had their home, have been omitted. Only those features which seemed to be of clinical and practical significance, have been correlated.

Sex Differences

As in other mental hospitals of the region, the *preponderance of females over males* represents a special problem in chronic patients. The ratio of 1·5/1 female to male is comparable with that found by Cross for the whole region (1954). The difference is partially accounted for by the longer life expectation of females. Ratios female to male increase rapidly with age so that the ratio for the over 70's is 2·3/1. One has also to think of clinical and social factors which may be responsible for this discrepancy. A male patient is probably more readily tolerated at home, particularly if he is able to earn a living even at reduced capacity. The situation is more difficult if the patient is female with

poor ability for household duties and impaired relationships with the family and with the neighbours. There are possibly cultural factors which influence the family and make them more ready to accept the return of mentally handicapped male patients. That these speculations are correct cannot be proven from the survey; but they are put forward as explanations to be confirmed by a more detailed study.

Another factor to be considered in this connection is the established preponderance of *women with affective psychoses* particularly in the higher age groups. Logan (1956) showed that there were nearly double the number of females admitted with these disorders in England and Wales.

Length of Stay and Re-admission

The distribution of *length of stay* shows that this hospital suffers, like many others, from a large number of chronic patients accumulated before the last war, i.e. before the methods of active physical treatments were introduced or fully applied. Of the cases (71 per cent.) who had been in the hospital for 10 years or more, almost two-thirds were never discharged and had become chronics after their first admission; during the last 10 years this proportion has dropped by 18 per cent. This suggests the more active treatments introduced during the last 10–15 years have led to a smaller number of “chronics” being accumulated in spite of an increasing admission rate of the hospital. It also implies a higher rate of discharge and consequently a greater population at risk for later re-admission.

Though not established from the figures of this survey there is general agreement about the rise in *re-admission rates* in recent years. It has been implied by some observers that there is little point in discharging a patient who will almost certainly require re-admission in the near future. Such a view is to be deprecated. A few months outside hospital is better than none and if a patient is not given every chance of life outside hospital one cannot be certain that he will be unable to adapt himself to life in the community. Clinical experience shows that predictions as to whether or not a patient can live outside hospital are not always correct. The best policy, where no contra-indication exists, is to encourage every opportunity of discharge. This applies not only in the affective disorders where the phasic nature of the illness makes repeated discharge and admission inevitable, but also to the much larger groups of schizophrenics where prolonged and repeated efforts to stabilize the patient outside hospital are needed. Critics of the rising re-admission rates should remember that from the purely administrative point of view a single bed which serves only one permanently hospitalized patient may also serve two or three patients who are discharged and re-admitted repeatedly.

It would be rash to conclude that the *modern methods of therapy* are preventing chronicity of mental illness altogether, and especially of schizophrenia; it seems, in fact, probable that neither insulin-coma treatment nor E.C.T. has any long-term influence on the course of this disease. What these and other treatments, such as the newly introduced drugs, achieve is to give the patient in remission an opportunity of getting back into the outside world and of adapting to a life among normal people, even if they still show more or less conspicuous symptoms of their illness.

Manifest Behaviour and the Problem of Discharge

The results of our attempt at *behaviour rating* for each patient were not entirely satisfactory. The traits used did not cover all significant aspects of

behaviour adequately and individual items did not yield much helpful information. The omission of a special item for incontinence was later regretted. It was found in many cases that in the time available it was difficult to rate the manifest behaviour except in negative terms such as affective indifference, apathy and social withdrawal. Patients whose behaviour presented actual difficulties to the nursing staff were more easily assessed. The fact that more than half the patients did not present any marked behaviour problem suggests that retention in hospital depends on social and other aspects unrelated to their behaviour pattern. This is in keeping with the clinical observation that some patients discharged from hospital at the request of their relatives are more disturbed in their behaviour than many who have to remain because their families are unable or unwilling to accept them home.

One difficulty in making any generalization from the behaviour ratings of these patients is that the observed behaviour may be purely *situational*. It is not uncommon in clinical practice to observe patients who behave well in hospital and yet show a totally different picture when allowed to return home on parole; in other cases these phenomena are reversed. Much would seem to depend on the *emotional atmosphere* of the home and the hospital, and the patient's attitude to either. The chronic mental patient is not unresponsive to his environment and the behaviour pattern may vary accordingly.

One strong influence on the patients' behaviour has not been active in the hospital to which this survey refers: there has been almost no treatment by *prefrontal leucotomy* of chronic psychotics, an operation carried out in more than 10,000 patients in the United Kingdom. It would be very interesting to compare our findings with those of a hospital in which neuro-surgical treatment was applied liberally in suitable cases.

The area served by the hospital is an industrial one with a long tradition of full employment for the majority of able-bodied and able-minded members of the family. Many mothers of large families do part-time factory work. To accept back into the family a mentally-handicapped member who, though not showing any really troublesome behaviour, is not a wage earner, appears to be a burden that some families are not prepared to accept. The problem of the return home may be greater than presented by the mere presence of the patient, as a wage-earning relative may have to take time from work to attend to his needs.

Another factor is the housing shortage; patients are not welcomed readily into already overcrowded accommodation. The duty of the family to care for its handicapped members has not been strengthened by the provisions of the welfare state. There are no incentives beyond moral ones for families to accept chronic mentally handicapped patients back again. This is particularly true when family ties have been weakened by prolonged hospitalization. By and large it would seem that the longer a patient is in hospital the more difficult it becomes to discharge him and that this is far more dependent on social factors than the actual condition of the patient.

There is, of course, another side to this problem of the best disposal for the permanently mentally handicapped patient. Some families extend themselves beyond reasonable limits to keep a chronic psychotic out of hospital to the detriment of the whole family and particularly the children. Outside the mental hospital and the family there are very few disposals for the permanently handicapped but relatively stable mental patient. It is unfortunate for this discussion that the social survey of the homes of these patients could not be carried out so that more precise formulation of the social factors involved could be made.

Physical Disorders and Nursing Care

The high incidence of patients with *physical disorders* indicates that many patients have a double handicap which may contribute to the necessity of retention in hospital. This would appear to be of particular importance in the older age groups. The physical factor may prevent gainful employment and make the problem of discharge far more difficult than it would be otherwise. The need to diagnose and treat accompanying physical disease in the chronic mental patient may influence policies of placing chronic mental patients in long-stay annexes with minimal medical and nursing care. In fact, doubly trained nurses may be needed for this type of case.

Our assessment of the *amount of nursing care* was obviously influenced by the attitude of the ward nurses who made the ratings. There seemed to be good agreement about patients who required more than routine attention. On the other hand, it was more difficult to decide whether a patient should be placed in the category of "routine care" or of "no nursing supervision". It was not only the attitude of the nurse to her charges that counted, but also the ward in which the patient happened to be placed. Some wards containing a majority of well conducted patients had a low nurse/patient ratio and in these most patients were expected to care for their own personal needs (toilet, feeding, dressing, etc.). In such wards the rating for patients who did not require nursing care was high. On the other hand, wards containing a high proportion of disturbed patients tended to have higher nurse/patient ratios. Here patients were given "routine care" irrespective of their individual needs. In such wards few patients were rated as needing no nursing care. This statement is not intended to imply criticism of the nursing staff. Except for patients requiring special nursing attention, the nursing need of a particular ward is inevitably determined by the needs of the majority. In large wards with 80 patients or more little heed can be given to the individuals not requiring special care. It is difficult when dealing with large numbers to make exceptions in ward routine so that patients who might care for themselves are given an opportunity to do so. This points to the need for proper classification of chronic patients according to their individual nursing requirements. Much of the trouble caused in chronic wards is due to the resistance against adequate grading. Sometimes this comes from the patient who attaches himself to one ward or one nurse or from the nurses who dislike to see their personal preferences for patients overruled.

The number of patients rated as requiring no nursing care underlines the point already made that retention in hospital depends not only on biological factors in the illness but on social ones as well.

The difference in nursing care required by males and females may be partially accounted for by bias of the raters, but is also probably influenced by cultural factors. Men are not as a rule reared to become highly efficient in caring for their purely domestic needs but to earn a living, whereas in women the opposite generally obtains. These cultural differences are often reflected in the wards themselves; female wards tend to be cleaner, fresher, and savoured with flowers; a less orderly state of affairs permeated with tobacco smoke often exists in male wards.

Employment of Patients

Some inferences can be made from the figures about the *employment of patients*. The large number of patients employed on domestic duties in the ward suggests that such employment cannot be realistic, as similar tasks could be

performed by very much smaller numbers of ward maids. There are good reasons to believe that the employment of patients outside the ward is dependent more on the work available to patients than the capacity of patients to do it. If occupation, beyond handicraft work provided through occupational therapy, is considered to be of therapeutic importance for the chronic patient, then the need for further developments in this field is clear. The idea of preparing the patient for work that he may take up after discharge, in other words, of rehabilitation and preparation for life outside hospital, only slowly takes root in the field of mental illness.

In view of the continuing problem of chronicity and of the striking overcrowding especially in the female wards of our mental hospitals, the attention of psychiatrists should be drawn to imaginative schemes of occupation and rehabilitation, particularly for women. At present, they are for a good part of the day unoccupied after sharing the available household duties in the ward, brooding about their complaints or withdrawn into a delusional world, living in an atmosphere of boredom and hatred which the nurses are unable to improve because of the large numbers under their care.

If there were the possibility of placing suitable cases in hostels and homes run by the Mental After Care Association, whence they could go to work or make themselves useful caring for the other boarders, this would be a step in the right direction. In fact, as a great number of chronic schizophrenics are physically well, all types of intermediary stages of care between hospital on the one end and family life on the other should be tried out and made use of.

Mobility and Degree of Supervision

The findings on the *mobility* of patients are in accordance with expectations. Only a small proportion of patients are bed-ridden and most of these are senile patients with accompanying physical disorders. The vast majority of patients are fully ambulant and it is with these patients that the bulk of the nursing is concerned. For most chronic patients the bed has no more significant role than a place to sleep at night. It seems illogical therefore that many mental hospitals have adopted the tradition of the general hospital in giving the bed pride of place. Day room and other facilities assume far greater importance in the treatment of the ambulant patient and doubts are cast on the usefulness of large bedded wards for the treatment of the chronic mental patients. Like normal people, these people sleep better where there is less noise. The disturbance caused by a noisy and difficult patient may influence the temper and emotional tolerance of several dozen of his co-patients sleeping in the same ward.

The *degree of supervision* accorded to patients suggests that there is more freedom and less supervision for male patients. The availability of work outside the wards for male and female patients and of staff to look after them may have contributed to this difference. The amount of freedom which a patient enjoys does not depend only on his mental state, but on the tradition of the ward in which he is placed. It depends moreover, on the overall administrative policy of the hospital. Since the survey was carried out, a number of wards have been unlocked and an increasing number of patients allowed freedom in the grounds without supervision. This makes it even more important that the patients should be adequately classified or in other words they should be placed in wards where they can be granted the most practicable amount of freedom.

There has been much emphasis of late on the "*open door*" policy in mental hospitals with a maximum of freedom for patients. While the advantages of

such policy have been clearly demonstrated, there seems a danger that this cult of the unlocked door may be overdone and thereby fall into disrepute. It is clear from this survey that some chronic mental patients remain highly aggressive and difficult, and probably dangerous to themselves or others. For such patients the "open door" in its accepted sense would sooner or later prove disastrous, particularly for a hospital situated in a densely populated urban area. The survey confirms the need to keep a *small* number of chronic mental patients confined to the ward or under constant supervision. The value of a more liberal regime for these few patients is doubtful even if it were practicable.

Visiting of Patients

The finding that two out of five chronic patients have no regular *visitors* underlines the isolation from their families and the outside world for many patients. The chance of discharging such patients is dependent on getting them sufficiently well to be able to earn their living and maintain an independent existence without family help and in competition with normal people. Few patients who have been in hospital for more than two years can measure up to such demands.

The other side of the picture is more heartening in that approximately *one-third of patients* who have been in hospital for *more than twenty years* still continue to receive either weekly or monthly visits. The families of such patients who continue to visit regularly and frequently over many years must obviously retain some concern about the patients' welfare. The fact that such patients have not been discharged means that either the patient's mental state does not allow life outside mental hospital or that there is some other obstacle, financial, domestic or housing, to prevent this. It seems highly probable that in some cases where strong family ties remain, discharge would be possible if some realistic and practical aid were available to help the family in dealing with the patient at home. The recent work of Poole and his associates in Oldham (1956) is of particular interest in this respect.

Diagnostic Assessments

The *diagnostic assessments* are of considerable interest, showing that numerically and therapeutically the greatest problems are the *schizophrenic disorders*; these far outweigh all the other diagnostic categories put together. As already pointed out earlier, all scientific and practical effort should be concentrated on these disorders. Although they have so far resisted preventive or therapeutic methods aimed at reducing the prevalence of the illness, enough is now known of the factors influencing behaviour and adaptation of the chronic schizophrenic to deal with his problems in and out of hospital. The large number of passive, indolent and harmless chronic schizophrenics as demonstrated in our survey are always in danger of being forgotten (Bickford, 1955) while the main resources of the hospital are devoted to recent admissions. The survey shows that there is no foundation for regarding the chronic mental patient as essentially different or requiring much less medical or nursing care than the more recent case. On the contrary, to avoid chronicity and utilize every sign of improved behaviour and better adaptation which may lead to discharge, well trained nursing staff and specialized knowledge is as much needed as in the treatment of earlier patients. It is in the nature of schizophrenia that only persistent attempts to adjustment, not easily discouraged by initial failure can lead to a satisfactory result.

The preponderance of females among *affective psychoses* has been mentioned already. The high incidence of accompanying physical disorders in the chronic affective psychoses, which was particularly marked in the aged, is in keeping with the clinical concept that the prognosis in these cases is significantly worse where physical disease is present. The fact that over two-thirds of the affective disorders were 60 years of age or more confirms the adverse factor of age on prognosis stressed by Watts (1956) and others.

The presence of 61 *mental defectives* in a mental hospital reflects on the inadequate provision for their care in mental deficiency institutions. New legislation can be expected for the care of this type of case when the report of the Royal Commission has been published. One can hope it will take account of the defectives' frequent inclination to psychotic episodes, and also of the many cases in which physical handicap is combined with mental subnormality and makes social adaptation a very difficult problem—as in the patients of our survey.

Chronic epileptics with character abnormalities and persistent or transient psychotic features represent a considerable burden for the nursing staff, in spite of their small numbers. It is difficult to see where else they could be cared for because they are unsuited for life in an open epileptic colony, even if such existed in the Region. They would probably be happier and less troublesome in small groups on a farm or in an Occupational Hostel.

The *senile psychoses* do not present a sizeable problem among the chronic patients. This finding is consistent with that of Roth (1955) who showed that 82 per cent. of these patients died within two years of admission. While the senile psychoses may well represent a serious problem among recent admissions they make a very small contribution to the chronic population.

SUMMARY

A comprehensive survey has been made of 442 male and 654 female patients who had been continuously in residence in a mental hospital for two or more years at the time of the enquiry. Details which were recorded are given in the Appendix.

Examination of age and sex distributions showed that the female to male ratio (1.5:1 for all ages) increased with age and the value for the 70 years and over age-group was 2.3:1. Possible explanations of these differences are discussed.

Two-thirds of all male and three-quarters of all female patients had been in residence for 10 years or more. Of these patients, 65 per cent. had been admitted only once as compared with 47 per cent. for those patients who had been in residence for less than 10 years. This suggests that a smaller number of chronic cases have been accumulated during the past ten years in spite of an increasing admission rate. Modern treatments did not prevent chronicity altogether, especially as regards schizophrenia.

Many patients did not show any serious behaviour problems and retention in hospital often depended on social and other factors unrelated to biological factors in the illness.

Almost one-third of the patients had physical diseases of note and the rate for those aged 70 years and over was outstandingly high. Thus many patients have a double handicap which may contribute to their retention in hospital and which may prevent gainful employment.

The need for proper classification of chronic patients according to nursing needs is stressed. From the four-fold classification used it was found that only very small proportions of patients required constant vigilance and about two-thirds required routine care only. It was clear that the degree of supervision accorded patients depends not only on their mental state but on ward tradition and administrative policy.

Nearly half the patients were entirely unemployed and of the remainder many were engaged on domestic duties on the wards; such employment is likely to be nominal and spasmodic. Employment and occupation for chronic patients presents special problems requiring imaginative schemes for their solution.

The large majority of patients were ambulant the whole day and the proportion of bed-ridden patients is highest in the older age-groups. Bed-ridden patients need more nursing care than do ambulant patients and 72 per cent. of male and 93 per cent. of female bed-ridden patients had physical diseases.

A further study of patients with certain diagnoses was made. Chronic schizophrenia accounted for 72 per cent. of male and 66 per cent. of female patients. The large majority of such patients had been in residence for 10 years or more and it is towards this disorder that

scientific and practical efforts should be directed. Schizophrenic patients, especially those in the younger age-groups make considerable demands upon nursing staffs; more than one-fifth had accompanying physical disorders. Almost two-thirds of patients with affective psychoses were 60 years of age and over and more than one-third had physical diseases superimposed upon their mental disorder. More than half the patients with a principal diagnosis of mental deficiency had a secondary diagnosis of mental disorder and half the male and one-third of the female mental defectives had physical diseases. Senile psychoses did not present a sizeable problem in chronic patients.

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APPENDIX

PROFORMA

Survey of Long-stay Patients

PART I

Name of patient.....

1-5 Registration number

--	--	--	--	--

6 Sex 0 Male 1 Female

7-8 Age of patient at time of survey (years)

--	--

Date of last admission to hospital.....

9-11 Duration of present stay in hospital

--	--	--

years months

12 Method of referment	1 Voluntary	3 Temporary
(Ring as appropriate.)	2 Certified	4 Other

13 Marital status. (Ring as appropriate.)

1 Single	3 Widowed	9 Not known
2 Married	4 Divorced or apart	

14 Number of previous admissions

11

Address of patient prior to admission.....

PART II

15 Ward Number

16 *Mobility of patient.* (Ring as appropriate.)

- 1 Bed-ridden (persistently).
- 2 Ambulant for part of day.
- 3 Ambulant for whole day.

APPENDIX—continued.

17 *Degree of supervision.* (Ring as appropriate.)

- 1 Confined to ward entirely.
- 2 Confined to ward except for messages, meals, etc.
- 3 Allowed outside ward under supervision.
- 4 Allowed outside ward without supervision.

18 *Week-end parole.* (Ring as appropriate.)

- 0 No parole
- 1 Occasional parole
- 2 Regular parole

19 *Employment*

- 1 Not working.
- 2 Working on ward (specify type of work)
- 3 Working elsewhere in hospital (specify work)
- 4 Working outside hospital (specify work)

Specify number of hours worked per day, if applicable.

For office use only

20-23 *Patients abnormal behaviour*
(Ring all appropriate letters.)

- A Depressed
- B Apathetic and inactive
- C Inaccessible
- D Alternating state
- E Irritable
- F Suicidal
- L Aggressive and/or destructive

- M Restless
- N Demented
- P Untidy
- R Overtalkative
- S Continually undressing
- T Others (specify)

24 *Nursing supervision required*

- 0 No nursing care of any kind.
- 1 Routine—supervision at meal times, dressing, washing, etc.
- 2 Routine plus special attention not to extent of 3 and 4 below.
- 3 More or less constant vigilance.
- 4 Constant vigilance plus restriction from violence.

25 *Physical diseases present.* (Ring as appropriate.) 0 No. 1 Yes.

If yes, specify

26-29 *Principal diagnosis at time of survey (W.H.O. Code)*

Specify

30 *Subsidiary diagnoses.* (Ring as appropriate.) 0 No. 1 Yes.

If yes, specify

PART III

Name(s) and address(es) of relative(s) and friend(s). Specify each person recorded and code relationship to patient by recourse to code at foot of page.

31 (i).....

32 (ii).....

33 (iii).....

APPENDIX—continued.

- 34 *Is patient visited by (i) above?* *Date of last visit*
 (Ring as appropriate.)
 0 Not at all 2 Weekly 4 3-monthly
 1 Occasionally 3 Monthly 5 Annually
- 35 *Is patient visited by (ii) above?* *Date of last visit*
 (Ring as appropriate.)
 0 Not at all 2 Weekly 4 3-monthly
 1 Occasionally 3 Monthly 5 Annually
- 36 *Is patient visited by (iii) above?* *Date of last visit*
 (Ring as appropriate.)
 0 Not at all 2 Weekly 4 3-monthly
 1 Occasionally 3 Monthly 5 Annually

RELATIONSHIP CODE

- | | | |
|----------------|------------|-------------------|
| 1 Wife/husband | 5 Brother | 9 Nephew |
| 2 Mother | 6 Daughter | 10 Cousin |
| 3 Father | 7 Son | 11 Other relative |
| 4 Sister | 8 Niece | 12 Friend |

THE INFLUENCE OF RHYTHMIC DRUMBEAT STIMULI UPON THE PULSE RATE AND GENERAL ACTIVITY OF LONG-TERM SCHIZOPHRENICS

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IN our clinical ward programmes we have utilized music rhythm instruments in rhythm groups to stimulate long-term schizophrenic patients and to induce their participation in social group endeavour (24). These clinical experiences together with various observations in this same connection, heightened our interest in the actual physiologic and motoric response of the schizophrenic patient to music rhythms. The evidence is strong that mentally "normal" persons do respond behaviourally and physiologically to music and its rhythms (7, 8, 11). Does the long-term *schizophrenic* patient manifest an objectively measurable response to such rhythm? If it could be demonstrated that the activity and the physiological functioning of the schizophrenic did respond to musical rhythms, then these stimuli might constitute a valid method for the modification of the behaviour of the schizophrenic patient.

The two measures chosen for the present study were pulse rate and general motor activity.

Pulse Rate

The cardiovascular system in mentally normal (non-psychotic) persons does respond to musical stimuli. Diserens (7) reviewed all studies in the world literature which had explored the human physiological responsiveness to music prior to 1923. Among these were Dogiel (1880) who demonstrated on dogs and on human beings that cardiac contractions were accelerated and the blood circulation definitely influenced by whistle stimuli on diapasons of varying pitch and by a Schubert serenade on violin, clarinet, and flute; Mentz (1895) who found that the pulse rate slowed or accelerated in accordance with the passiveness or activeness of the listener's attitude as he attended to the music; Binet and Courtier (1896) who ascertained that complex melodic stimuli such as the Marche Triomphale from Tannhäuser and La Rencontre (chant) from Faust caused the pulse rate to show a general rise; Shepard (1906) who wrote that agreeable and exciting music increased the heart rate. Weld (29) reported that the degree of cardiac acceleration was not correlative with the musical tempo. Slow tempos produced an increase in pulse rate fully as great as that produced by fast tempos. That music affects the heart rate has also been concluded by other investigators although these findings are not fully compatible in terms of the differential effects (increment vs. decrement) of varying types of music upon the pulse rate (15, 27, 28, 29).

Many of the studies in this field have been clinical in nature; hence they were necessarily weak in experimental design, adequacy of controls, or size

of sample. One important and well-planned experiment by Ellis and Brighthouse (9) did not yield consistent nor significant mean change in the heart rate of a group of 36 college students who listened to selected recorded music during 4-minute intervals. The heart rate of some subjects did rise, but the significance of this change was not statistically tested. On the other hand Gilliland and Moore (13) in their comparison of recorded classical with jazz music concluded that jazz did increase the heart rate significantly above that of classical music.

These findings were obtained from mentally normal subjects. The cardiovascular effects of musical stimuli upon the mentally disordered or the schizophrenic patient have not yet been submitted to controlled experimentation. The variable, unpredictable nature of the schizophrenic response to stress and to sensory stimuli of all types is well known. The present report describes an investigation into the effect of drumbeats upon the pulse rate of chronic schizophrenic patients.

Problem 1

(a) Does music in the form of rhythmic percussive drumbeats delivered to a group of long-term, hospitalized, deteriorated male schizophrenic patients significantly affect their pulse rates when compared to the ordinary changes which occur during a control period without such beats (designated as "Silence")?

(b) Do different rhythmical rates of drumbeat stimulation (different basic frequencies of beat) exert a differential effect upon the pulse rates of these patients?

General Motor Activity

A second portion of this investigation pertains to the motor response of long-term schizophrenic patients to musical stimuli. A variety of techniques has been employed to define the effect of musical stimuli upon the neuromuscular response of mentally "normal" persons. Diserens (7), in his summation of this research to 1923, concluded that music unquestionably did affect the output of muscular energy. The research literature since then has been steadily and cumulatively emphasizing this point. Some have explored the generalized, undirected motor response to music stimuli. Others have studied the focussed or directed motor response as in reaction time experiments, dynamometer pull, or creative-productive work output (8, 12, 14, 18). Various aspects of the neuromuscular apparatus have been submitted to study by assorted methods (8, 9, 11), and some work has been reported with brain-damaged but non-psychotic subjects (4, 10, 19).

There is a dearth of studies pertaining to the motor response of mentally disordered or psychiatric patients to music stimuli. Altshuler and Shebesta (1), in a pioneer investigation of the effect of music upon the motor output of several excited psychotic patients during hydrotherapy, tentatively concluded that music could produce favourable psychological effects upon psychotic patients by altering their general motor output. Skelly and Haslerud (25) recently studied the effect of music upon the general activity of apathetic schizophrenics. Their subject population consisted of 39 females, aged 19 to 56 and showing "blunted emotionality, lack of feeling, and impassivity", but no intellectual deterioration. All had been admitted to the hospital within the past year and were characterized by chronic inactivity. The general activity of these patients during assignment to an occupational therapy room, with and without accompanying music, was rated on a 7-point scale. The authors reported a significant increment in the

general activity of these schizophrenics when lively recorded music was played to them.

Problem 2

(a) Does music in the form of rhythmic percussive drumbeats delivered to long-term, deteriorated schizophrenic patients, significantly alter their level of general activity when compared to a control period without such rhythmic drumbeats?

(b) Do different rates of rhythmic stimuli, i.e. different frequencies of rhythmic pulsation, exert a differential effect upon these patients' level of general activity?

PROCEDURES

Subjects

These consisted of all the ambulatory schizophrenic patients on two wards which housed long-term mental patients. There were 23 subjects in total. All were World War I, white, male veterans, median age 60 years, and had been hospitalized for long periods (see Table I). Their median educational grade level was 8th grade according to the entries in their past clinical records, but this may be erroneously high in view of their generally low socio-economic background. Their schizophrenic reaction types were: 10 paranoid, 7 hebephrenic, 4 catatonic, 1 simple, and 1 chronic undifferentiated. The majority had been unskilled or semi-skilled labourers prior to hospitalization; several had held clerical positions.

TABLE I
Description of Experimental Subjects (N=23)

Description	Median	Lower Quartile (Q ₁)	Upper Quartile (Q ₃)
Age (years)	60	57	64
Education (grade)	8th	7th	8th
Age at first hospitalization	31	26	35
Years in hospitals	29	16	34

Stimuli

The three experimental stimulus rhythms each consisted of a bongo drum solo by a professional drummer.* Each one of the three was made at a different basic rhythmic beat, timed by a clocking system. The basic frequencies or beats selected were designated as follows: (a) Rhythm A at 54 per minute, representing a low pulse frequency with great enough deviation from the common or normal pulse rate to be perceptibly slower but still within the range of average pulse variability; (b) Rhythm B at 90 per minute, representing a pulse rate great enough to be perceptibly higher than the common pulse rate of 72 per minute, but still within the range of average pulse variability; and (c) Rhythm C at 72 per minute representing the common pulse rate. The bongo drum solos consisted of varied patterns within the stated frequencies and were therefore definable as music. They avoided the monotonous, unvarying, amusical sounds of simple metronomic beats per minute. The three different rhythmic stimuli are hereafter

* Grateful acknowledgment is made to the Hospitalized Veterans Service of the Musicians Emergency Fund, Inc., New York, for providing the drum soloist and the necessary materials required to prepare the stimulus rhythms of this experiment. Dr. Thomas Gilmore is thanked for his suggestion that these three beat frequencies be used, for his recording of the stimuli and their broadcast into the experimental ward, and for his participation as an observer.

termed Rhythm A (54/minute), Rhythm B (90/minute) and Rhythm C (72/minute). Rhythms A, B and C respectively, each constituted one of the three experimental conditions. A fourth condition, the control condition, was termed "Silence"; it designated the absence of rhythmic musical stimuli. The stimulus rhythms were recorded upon magnetic tapes; the rhythm appropriate to each experimental condition A or B or C, was supplied from the tape play-back through the loudspeaker of the ward television system. The picture tube of the television was of course turned off during these periods. All volume settings were maintained at constant readings.

The experimental and control conditions (A, B, C and Silence) were presented in the following sequence:

	<i>First Day</i>	<i>Second Day</i>	<i>Third Day</i>	<i>Fourth Day</i>	<i>Fifth or Last Day</i>
	Tuesday	Wednesday	Thursday	Friday	Monday
10.00 a.m.	Silence	Rhythm A	Rhythm C	Rhythm B	Silence
2.45 p.m.	Silence	Rhythm B	Rhythm A	Rhythm C	Silence

Another and final experimental condition was introduced for theoretical purposes. It is included below under the section on Results, and is reported there in several tables. This was the experimental condition "Combined Rhythms (A, B, C)". It did not represent an experimental session or sessions as such. It was the mean of all the readings taken individually under Rhythm A and B and C respectively. This "Combined Rhythms" condition permitted statistical tests to be made between the effects of Silence and of Rhythm regardless of the specific tempos (within the limits of the beat frequencies of this experiment).

Procedure

All patients participating in the study were brought into a large ward day room mid-morning and early afternoon of each day that the experiment was in progress. The other, non-participating patients were removed from the ward during these sessions. The particular day room selected was well known to all experimental patients. For over half the experimental group it was the day room of the ward within which they ordinarily lived. At each of these experimental periods the patients were permitted to take their ease and assume the position most comfortable to them before the pre-session or base-line experimental readings were recorded by the observers. Upon the conclusion of each experimental session the necessary post-session readings were noted and the patients were then returned to their usual activities for the day.

Each patient served as his own control. The control observations, pre- and post-, were made during the sessions of Silence (absence of rhythmic drumbeat stimuli) in accordance with the sequence described earlier under *Stimuli*.

The same experimental situation was otherwise maintained for the condition of Silence as for the three conditions of rhythm.

Observational Methods

The measurements were made by a battery of nine observers including nurses, nursing assistants, a social worker, a psychiatrist, and two psychologists. Each observer recorded the pulse rate reading and motoric activity reading for a minimum of two or a maximum of three specifically designated patients (total $N=23$), throughout the course of the experiment. This permitted him to utilize a constant frame of reference for the notations of motor behaviour and activity changes in each of his specifically designated patients.

The ratings of activity level, and change in activity level, were made on a specially prepared scale. A fresh scale was utilized for each subject at every session. This simple scale called for the designation of the Initial Activity Level at the start of the session (pre-session). Direction and extent of Change in Activity Level was designated at the conclusion of the session (post-session). Space was also provided for qualitative comments.

ACTIVITY RATING SCALE

Initial Activity Level (please check one):

- _____ (1) Quiet
- _____ (2) Moderate Active
- _____ (3) Very Active

Change in Activity Level (please check one):

- _____ (+2) Activity Much Increased
- _____ (+1) Activity Increased a Little
- _____ (0) Activity Unchanged
- _____ (-1) Activity a Little Less
- _____ (-2) Activity Much Less

Comments: (Continue on back if necessary)

Pulse rate readings were taken for each patient after a 5-minute "settling down" period immediately prior to the start of the experimental or control session. The pulse rate was again taken at the conclusion of the session, twenty minutes later. A typical session, as for example that of Wednesday morning, proceeded according to the following sequence:

- 9.50 Experimental subjects brought into ward day room.
- 9.55 Initial Activity Level checked and pulse-rate readings taken for each subject.
- 10.00 Rhythmic stimulus broadcast into ward (Silence, if control condition).
- 10.00- Observations made by observers, from nursing station and kitchen
- 10.20 area which commands a view of the entire ward day room.
- 10.20 Degree of Change in Activity Level checked. Pulse rates taken by the several observers.
- 10.25 Patients returned to usual activities for the morning.

Behavioural Ratings

All patients were rated for their level of behavioural ward adjustment during the three weeks preceding this experiment. The ratings were made individually by each of the two nurses assigned to these patients' wards and therefore well acquainted with their daily behaviour. The behavioural rating scale was valid for patient populations such as this (23). It was simple, concrete, and readable; it described behaviour in six major areas of adjustment; Self Care, Orientation, Communication and Socialization, Psychotic Behaviour, Co-operation, and Reaction to Environment. Scores could range from a minimum of 0 (lowest adjustment level) to a maximum of 100 (highest adjustment level).

The two scores of these behavioural rating scales, obtained from the two nurses, were averaged for each patient and gave a quantitative measure of the ward adjustment level for that patient. These measures permitted statistical tests to be made between the general behavioural adjustment of the patient and his response to the several experimental or control conditions.

Methodological Comments

For the purpose of this study, motor activity included all forms of activity (or lack of same) whether verbal, locomotive, or manipulative. Macroscopic muscular movement was the criterion. The purpose of the behaviour, its meaningfulness or its congruence with reality, was not at issue in this connection. The observers were instructed that "activity pertains to any form of motor behaviour including change in position, walking, running, striking, talking, shouting, or other movements or gestures".

Prior to instituting this experiment a meeting was held with the observers to explain the procedures, ratings to be made, and role of the observer. Terms and definitions were clarified and examples given. The need was emphasized to maintain a constant frame of reference in judging the activity levels. The observers were briefly informed that the purpose of the experiment was to study the pulse rates and activity of schizophrenic patients in different situations. Hypotheses were not presented.

RESULTS

Pulse Rates

(a) The mean pulse rates and their respective changes under the experimental (Rhythm) and control (Silence) conditions are presented in Table II. The statistical significance of the change, pre- to post-, was tested under each condition (*t*-test). The results of these tests of significance are included in Table II. There was a decided mean drop in pulse rate during the control condition of Silence. No such significant drop occurred under any of the experimental Rhythm conditions. In the case of experimental Rhythm A there was even a small although statistically non-significant rise in pulse rate. Figure 1 portrays these changes graphically.

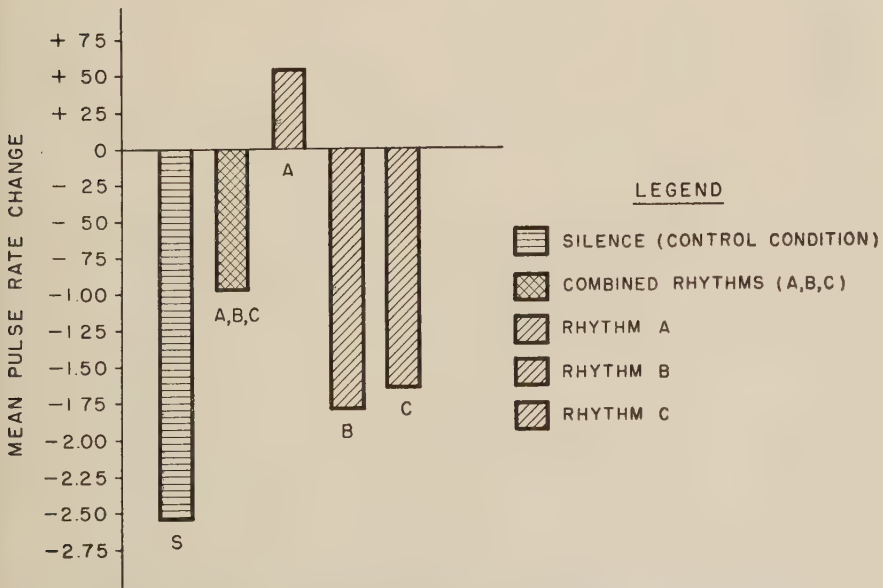


FIG. 1.—Mean pulse rate changes under the several experimental conditions.

TABLE II

Mean Pulse Rates and Their Responses to the Several Experimental Conditions (N=23)

Condition	Algebraic Means Direction of Change Considered				<i>t</i>	<i>p</i>
	Mean Pre-	Mean Post-	Mean Change			
Silence	77.84	75.29	-2.55		3.29	< .01*
Rhythm A	78.41	79.02	+0.61		1.94	.10
Rhythm B	77.00	75.13	-1.87		1.78	.10
Rhythm C	75.82	74.18	-1.65		1.68	.20
Combined Rhythms (A,B,C)	77.08	76.11	-0.97		2.00	.10

* Null hypothesis rejected at the indicated confidence level.

(b) The mean pulse change under each condition was compared with the mean pulse-rate change under each one of the other conditions. These comparisons, together with the results of their *t*-tests of statistical significance, are reported in Table III. The individual Rhythm condition, Rhythm A or B or C, was not tested against the condition of Combined Rhythms (A, B, C) since each of the former was already included in the latter and such a test would therefore have been invalid due to contamination.

TABLE III

Comparisons of Mean Pulse-Rate Changes Among the Several Experimental Conditions (N=23)

Conditions Compared	Pulse Rates			
	Mean Change	Difference	<i>t</i>	<i>p</i> Difference
Silence vs. Rhythm A	-2.55 +0.61	3.16	2.27	.05*
Silence vs. Rhythm B	-2.55 -1.87	0.68	.60	.60
Silence vs. Rhythm C	-2.55 -1.65	0.90	.86	.50
Silence vs. Combined Rhythms (A, B, C)	-2.55 -0.97	1.58	2.16	.05*
Rhythm A vs. Rhythm B	+0.61 -1.87	2.48	1.39	.20
Rhythm A vs. Rhythm C	+0.61 -1.65	2.26	1.57	.20
Rhythm B vs. Rhythm C	-1.87 -1.65	0.22	.13	.90

* Null hypothesis rejected at the indicated confidence level.

The mean pulse rate dropped significantly less under Rhythm A than under Silence ($p=.05$). It also dropped less under Rhythms B and C than under

Silence, but the difference was not statistically significant. Finally, comparing Silence with the Combined Rhythms (A, B, C), the mean pulse-rate drop was significantly smaller under Combined Rhythms (A, B, C) than under Silence ($p = .05$).

In summary, the conditions of Rhythm prevented the pulse rate from slowing down to that level which did occur under the control condition of Silence. Comparing the three respective experimental rhythmic conditions among themselves, there was no significant evidence of differential effects exerted upon pulse rate shift by these variations in beat frequency. It was true that the mean pulse rate did rise somewhat under frequency A while it dropped somewhat under B and under C, but statistical analysis gave no evidence that any factor other than chance was involved in this trend.

(c) The various mean changes in pulse rate reported and discussed above had been calculated in the usual algebraic fashion. That is, direction of pulse rate—whether up (+) or down (—)—was taken into account in the various computations. However, it was also theoretically possible that the *absolute* change of pulse rate regardless of direction (arithmetically calculated) might show a response to the experimental conditions. Such a response could conceivably have occurred and yet be obscured by the ordinary algebraic methods of computation which took account of direction of change. Hence the absolute or arithmetic mean pulse rate changes were also computed for the experimental and control conditions (Table IV).

TABLE IV
Comparisons of Absolute Mean Pulse Rate Changes Among the Several Experimental Conditions (N=23)

						Arithmetic Means Direction of Change Disregarded		
Condition						Mean Change	<i>t</i>	<i>p</i>
Silence ..	..	..	..	..	..	6.15	19.22	< .001*
Rhythm A ..	..	..	..	..	..	5.22	6.52	< .001*
Rhythm B ..	..	..	..	..	..	5.65	9.05	< .001*
Rhythm C ..	..	..	..	..	..	5.56	7.94	< .001*
Combined Rhythms (A, B, C)	..	..	..	..	..	5.48	13.91	< .001*

* Null hypothesis rejected at the indicated confidence level.

Within each one of the respective conditions there was a significant absolute mean change, pre- to post-condition ($p < .001$). Pulse rates were continually fluctuating, some up and some down, even within these brief 20-minute intervals. However, *t*-tests *between* the conditions revealed no significant differences nor even any trends: when absolute mean pulse-rate change was studied, there was no evidence of differential effects between the experimental and control conditions.

(d) It will be recalled that ward adjustment ratings (behavioural rating scale scores) had been made for each of the 23 patients by two nurses at the beginning of the experiment. The mean of these two ratings constituted the adjustment rating score for each of the patients. The group mean ($N=23$) was 35.4, $Q_1=23.0$ and $Q_3=45.5$.

Rank order correlations were computed between these adjustment ratings and the pulse-rate changes under each of the experimental and control conditions. Correlations were calculated both for (a) the algebraic mean pulse-rate

changes with direction of change, up (+) or down (—), taken into account, and (b) the absolute pulse-rate change regardless of direction. In no case, neither under the condition of Silence nor of Rhythm, was there a significant degree of correlation between the pulse-rate change and adjustment level. Examination of these individual correlations gave no reason to believe that the behavioural ward adjustment was differentially or causally related to change in pulse rate during Silence or Rhythm.

General Motor Activity

(a) The initial rating of general motor activity prior to each session was made according to the 3-step scale: (1) "Quiet", (2) "Moderately Active", (3) "Very Active". The medians of these initial activity ratings were equivalent under each condition, whether Silence or Rhythm. In each instance the initial group median rating (pre-) for that condition was "Quiet", with Q_1 at "Quiet" and Q_3 at "Moderately Active". Descriptively, therefore, the group might be characterized as "Quiet", but with occasional outbursts from individual members and containing a few patients who were continuously active and/or vociferous. Initial group mean scores were calculated from the assigned values "Quiet"=1, "Moderately Active"=2, and "Very Active"=3, under the assumption of equidistance between these three steps. The means were as follows: Silence=1.4, Rhythm A=1.3, Rhythm B=1.3, and Rhythm C=1.4. The initial pre-session motor status was therefore equivalent for all experimental and control conditions (no significant differences).

(b) The change in general activity level over the course of each session had been rated according to the following 5-step scale: "(+2) Activity Much Increased, (+1) Activity Increased a Little, (0) Activity Unchanged, (—1) Activity a Little Less, (—2) Activity Much Less." The measurements of change for the several sessions under each experimental and control condition respectively were combined for that condition, and their respective means computed. The statistical significance of the mean change under each condition was tested by *t*-test. Table V presents the findings. These means and statistical tests were based upon the assumption of equidistance between the five steps of the scale.

TABLE V
Mean Changes in Activity Levels Under the Several Experimental Conditions (N=23)

Condition	Mean Change (Pre- to Post-)	<i>t</i>	<i>p</i>
Silence	— .02	0.03	> .50
Rhythm A	+ .46	2.75	.02*
Rhythm B	+ .41	3.57	< .01*
Rhythm C	+ .28	2.19	.05*
Combined Rhythms (A, B, C) ..	+ .38	5.52	< .001*

* Null hypothesis rejected at the indicated confidence level.

There was a significant mean increment in the general level of activity during the Rhythm sessions. In decided contrast to this, the general level of activity remained essentially unchanged during the control Silence sessions. Figure 2 represents this graphically.

(c) The mean change in activity level under each condition was compared with the mean change in activity level under each of the other conditions respectively. These comparisons, together with their *t*-tests of statistical significance, are reported in Table VI. The individual conditions of Rhythm (A or B

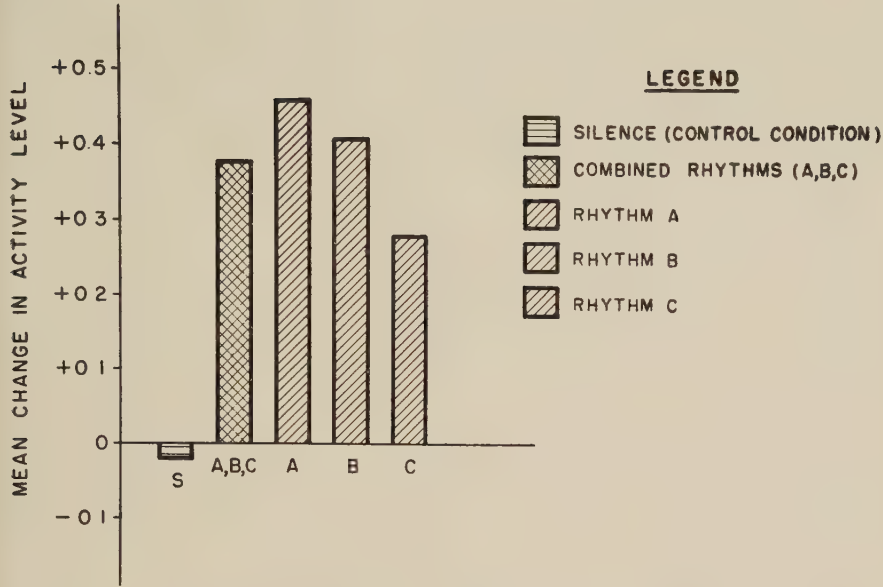


FIG. 2.—Mean changes in activity levels under the several experimental conditions.

or C) were not tested against the Combined Rhythm (A, B, C) since each of the former was already included in the latter and such a test would therefore have been invalid.

TABLE VI
Comparisons of Mean Changes in Activity Levels Among the Several Experimental Conditions (N=23)

Conditions Compared	Activity Levels			
	Mean Change	Difference	t	p Difference
Silence vs. Rhythm A	- .02 + .46	.48	3.12	< .01*
Silence vs. Rhythm B	- .02 + .41	.43	5.12	< .001*
Silence vs. Rhythm C	- .02 + .28	.30	2.21	.05*
Silence vs. Combined Rhythms (A, B, C)	- .02 + .38	.40	5.52	< .001*
Rhythm A vs. Rhythm B	+ .46 + .41	.05	0.22	> .50
Rhythm A vs. Rhythm C	+ .46 + .28	.18	1.07	.30
Rhythm B vs. Rhythm C	+ .41 + .28	.13	1.00	.40

* Null hypothesis rejected at the indicated confidence level.

Each of the experimental Rhythms was significantly more potent than Silence in its influence upon the level of general activity of these chronic schizophrenic patients. Since the respective Rhythms showed no significant differences when compared to each other, there was no reason to believe that any one Rhythm was more effective than the others in altering the general level of the patients' activity. Rhythmic *frequency* of beat, slow versus fast within the definition and limits of this experiment, did not influence the degree of change in activity level.

(d) The mean change in activity level during the course of the several respective conditions was also computed on the basis of *absolute* change (arithmetic mean) regardless of direction of the individual changes (see Table VII, column 2). These absolute mean changes were: Silence = .24, Rhythm A = .72, Rhythm B = .54, Rhythm C = .54, Combined Rhythms (A, B, C) = .60. In every one of these conditions the extent of absolute mean change pre- to post-session was statistically significant ($p < .01$). The implication was this: that even during brief 20-minute intervals these schizophrenic patients, the majority of whom were adjectivally described as "Quiet", did show statistically significant fluctuations (increase or decrease) in motor activity. Such fluctuations were present during the control condition Silence, and even more so during the experimental Rhythm conditions (Table VII). Table VII reports the results of statistical tests of the significance of the difference between the absolute mean changes (direction of change disregarded) in motor activity among the various experimental and control conditions.

TABLE VII
Comparisons of Absolute Mean Changes in Activity Level Among the Several Experimental Conditions (N=23)

Conditions Compared	Activity Levels		Algebraic Means	
	Mean Change	Difference	<i>t</i>	<i>p</i> Difference
Silence vs. Rhythm A	.24 .72	.48	3.72	< .01*
Silence vs. Rhythm B	.24 .54	.30	2.54	.02*
Silence vs. Rhythm C	.24 .54	.30	4.22	< .01*
Silence vs. Combined Rhythms (A, B, C)	.24 .60	.36	3.95	< .01*
Rhythm A vs. Rhythm B	.72 .54	.18	1.16	.30
Rhythm A vs. Rhythm C	.72 .54	.18	1.16	.30
Rhythm B vs. Rhythm C	.54 .54	0	0	> .50

* Null hypothesis rejected at the indicated confidence level.

In the case of Silence, where four readings (sessions) were taken on each of 23 patients to give a total of 92 readings, there were 20 changes in all. Of these 20 readings where activity level fluctuated, 10 went up in activity (+) and 10 went down (-). Thus, although the algebraic mean change under Silence was not significant (see Table VI), the *absolute* mean change under Silence reported herewith, and which reflects fluctuation regardless of direction, was statistically significant (mean = .24, $p < .01$).

The case of the several Rhythms was somewhat different. Their mean changes were statistically significant both for absolute means (Table VII) and algebraic means (Table V). The absolute mean changes under the Rhythms (Table VII) merely reflected the changes already reported in Tables V and VI, where direction of change (+ or -) had been taken into account in the computation. These changes under the Rhythms (Tables V and VI) had all been markedly upward in the direction of increment (+). Hence, for these Rhythm conditions the absolute mean changes of Table VII were not much different in interpretation from the algebraic means previously reported in Table V. For example, of 46 readings under Rhythm A, 23 showed positive (+) change and only 4 showed negative (-) change. Of 46 readings under B, 20 showed (+) change and only 3 showed (-); of 46 under C, 18 were (+) and 6 were (-); of 138 readings under Combined Rhythms (A, B, C) 61 were (+) and only 13 were (-).

Several inferences seem warranted. Even under ordinary control or Silence conditions, there is a surprising degree of change in the motor activity of quiet schizophrenics over the course of brief time spans (20-minute spans). Some show increase in their activity, others decrease. This constitutes a state of dynamic equilibrium in the sense that the numbers of patients increasing and decreasing in activity are equal in extent. They are in balance and therefore do not significantly change the algebraic group mean activity level. Rhythms, however, do significantly upset this equilibrium by driving the mean group movement emphatically forward into increased motor activity.

(e) Rank order correlations were computed between the patients' adjustment rating scale scores (behavioural adjustment) and their motoric changes under each of the experimental and control conditions. Utilizing the variable of absolute change in activity regardless of direction, two statistically significant rank order correlations were obtained: behavioural rating scale score with Combined Rhythms (A, B, C), $Rho = .57$ and $p = .01$; with Rhythm B, $Rho = .64$ and $p = .01$. Utilizing the variable of algebraic change in activity (direction taken into account), a significant rank order correlation was obtained between behavioural adjustment and Rhythm B: $Rho = .69$ and $p = .01$.

This provided some partial evidence that the degree of responsiveness to Rhythm did reflect the level of behavioural adjustment. Not so the degree of responsiveness to Silence.

(f) Statistical tests were made of the null hypothesis that the degree of motoric change was unrelated to the initial pre-condition status ("Quiet", "Moderately Active", "Very Active"). The results gave no reason to reject this null hypothesis. Similar statistical tests were made of the null hypothesis that the patients' behavioural ward adjustment was unrelated to their initial pre-condition status. Again, the results gave no reason to reject this null hypothesis.

Familiarization

The null hypothesis was tested that *familiarization* with the Rhythms did not lead to altered pulse rate responsiveness or altered general motoric

responsiveness. Mean scores under the Combined Rhythms (A_1 , B_1 , C_1) for the first half of the experiment were compared with the mean scores for Combined Rhythms (A_2 , B_2 , C_2) for the second half of the experiment. Statistical tests of the significance of the differences between the group mean scores for pulse rate changes, and again for motor changes, gave no reason to reject this null hypothesis in either instance. All probabilities were insignificant. Within the limits of this experimental design and procedure there was no reason to believe that the patients' increasing familiarization with the Rhythms affects their responsiveness of pulse rate or of general motor activity.

Correlations

A rank order correlation was computed between the algebraic mean changes in pulse rate and in motor activity under the several conditions of this experiment. The rank order correlation between pulse-rate change and activity change was $+0.68$, $p=0.05$. Under those experimental conditions which raised the activity level there was also a tendency for the pulse rate to rise, and vice versa. A causal relationship between motor activity and pulse-rate change need not necessarily be postulated to account for this relationship. It is alternatively possible that both variables reflect an underlying holistic response to the Rhythmic stimuli, a responsiveness which is reflected by a variety of physiological indices including the pulse rate and the neuromuscular activity.

DISCUSSION

The findings are recapitulated as follows: (a) The mean pulse rate dropped significantly during Silence but not during Rhythm, (b) Absolute mean pulse-rate change (direction disregarded) showed significant increase under all conditions including Silence; but there was no significant difference *between* any of the experimental or control conditions, (c) Behavioural adjustment level was not correlative with pulse rate change under the several conditions of Rhythm or Silence, (d) The pre-session motor activity status was characteristically "Quiet", (e) There was a significant mean increment in activity during all experimental Rhythms but not during the control Silence, (f) The activity state of these chronic schizophrenics under ordinary resting conditions—Silence—constituted a state of dynamic equilibrium rather than total passivity, (g) Rhythm exerted force upon this equilibrium, driving it forward toward increased activity, (h) The level of ward adjustment was correlative with the degree of motor responsiveness to Rhythm, (i) Familiarization with the rhythms had no effect upon changes in pulse rate or activity level, and (j) The degree and direction of pulse-rate change was correlative with the degree and direction of activity change, computed for groups.

Problems 1a and 2a, posed earlier, are now answered in the affirmative. Musical rhythm does significantly affect pulse rate and activity level, in contrast to the resting control state (Silence).

Problems 1b and 2b conversely are answered in the negative. There are no significant differential effects among the several rhythmic beat frequencies. The faster rhythms (B at 90/minute and C at 72/minute) are no more nor less effective than the slower rhythm (A at 54/minute) in affecting the pulse rate and activity level. It is true that the mean pulse rate drop (see Figure 1) and the mean change in activity level (see Figure 2) for Rhythm A are greater than for Rhythms B and C, but this does not attain statistical significance. If further experimentation were to yield a significant differential in favour of the slowest

beat frequency A, then this would indeed be in an unexpected direction. Where differential behavioural effects have been found to occur in the literature they reflected the music tempo and showed greater response to fast than to slow rhythms. However, in view of the actual statistical results obtained at this time there is no evidence that tempo of rhythm (54/minute vs. 72/minute vs. 90/minute) is a significant factor in the alteration of these variables of behaviour. It may be that the gradient of slow-to-fast employed in this study covers too narrow a range to be perceptibly or functionally different in its organismic effects. Perhaps extension upward and downward to greater extremes of tempo would yield differential effects not exerted by this narrower gradient. We are nevertheless constrained to conclude that within the limits of these experimental conditions the *tempo* of rhythm (beats/minute) does not significantly influence the pulse rate nor activity level. Rhythm *per se* is demonstrably an influential factor but the tempo of that rhythm—whether 54 or 72 or 90 per minute—is not.

The results indicate unequivocally that patterned rhythmic drumbeats do stimulate an organismic response in chronic, deteriorated schizophrenics who have been hospitalized for many years. Their neuromusculature responds strongly, and their cardiac rate is maintained at a higher-than-average level (for them). These objective data are entirely consistent with the findings of Skelly and Haslerud (25). They demonstrate that older, long-term schizophrenics can be stimulated to behavioural and cardiac response by musical rhythm: their many years of hospitalization and their deteriorated status notwithstanding.

The present study was concerned with motor and cardiac response as such, not with the creative quality or directed focussing of that response. A challenging problem flows directly from the findings of this investigation: Can the neuromuscular response of these schizophrenics be directed into creative, productive channels? Are we able to focus the energy increment of these patients, an increment which is released through the medium of musical rhythms, into a direction which is productive and which leads toward therapeutic success (increased behavioural adjustment)?

Research-wise, this might be investigated through the medium of occupational therapy. Under the influence of music or musical rhythm, will these long-term schizophrenics increase creative or constructive behaviour when craft materials are made available, materials which lend themselves to creative use? Industrial therapy might also provide the setting for such a study. Under the influence of music rhythm does the deteriorated schizophrenic patient become more productive in the quality or quantity of his work output?

Many of those same factors which influence the effect of music upon normals will undoubtedly prove to be significant for psychiatric patients. This is not to say that the patients will respond precisely as normals to equivalent elements of music. Their response cannot be predicted now before the application of detailed experimental procedures. Among the psychological and musical variables which must be considered are the educational and intellectual levels of the patients, their likes and dislikes in music, their familiarity with the selections, the manual activity in which they are engaged while listening, the volume and tempo of the music, the activeness or passiveness of the listening attitude, the mood of the patients and of the music, and finally the duration of the musical stimulation. Care must be exercised in this connection, for the distinct possibility exists that music can become a distracting influence which

is detrimental to creative production (2, 5, 26). The industrial use of music may give significant leads for its therapeutic application (3, 14, 20). Among the many individual differences which do affect response must also be counted the differences in physiology, in previous experience, and in psychological imagery type (21, 22). Since the response to music rhythm is so complex a phenomenon, the prediction of this response for individuals is fraught with difficulty. Nevertheless, such prediction to individuals is within the realm of possibility. A parallel situation existed in the psychological content area "level of aspiration"; with the accrual of scientific knowledge concerning this area it has become more and more possible to predict the individual level of aspiration with narrower margin of error (17).

Dearborn (6) submits arguments well adapted to the thesis that movement is necessary for normal life. Abulia is a deficiency of will power or conation; essentially, a default in the normal impulse to activity. Abulia is prominent in the schizophrenic process, which presents so glaring a default in the normal neuromuscular and motor responsiveness to stimulation. Musical rhythm demonstrably has power to stimulate the schizophrenic and to induce activity which parallels that of normals. Therefore it provides one possible avenue through which the schizophrenic might partially be drawn back to normality of response and/or behaviour. At the very least this possibility demands thorough exploration.

The mean fall in pulse rate during Silence was consistent with the patients' "resting" status. Had a longer interval than five minutes been permitted on the ward prior to each pre-session reading, perhaps 15-25 minutes, it is conceivable that the mean pulse rate might have remained unchanged under the control (Silence) sessions rather than falling as it did. Similarly, the mean pulse rate under the Rhythms might have been driven upward rather than being merely held back by Rhythm from undergoing so great a fall as under Silence. Since, however, all pertinent conditions in this experiment were equivalent for the control Silence as for the experimental Rhythm sessions, then the present findings are fully significant under the equable circumstances of this research design.

The rhythmic stimulus patterns employed in this investigation were complex and varied, containing unexpected accents. Repetition of this experimental design but with simply accented and repetitive metric rhythm might or might not yield the same results as those described above.

This report delineates experimentation with but two physiological measures. Numerous other measures are available, and many have already been utilized in music research with normals. Among these are plethysmographic recordings, EEG, spiograms, and electromyography. An extension of these to the study of schizophrenic responsiveness to music could prove fruitful beyond expectation.

SUMMARY

This study was designed to evaluate the response of deteriorated schizophrenics to rhythmic percussive drumbeats. The variables studied were pulse rate and general motor activity. Three different experimental drumbeat frequencies were utilized: Rhythm A at 54/minute; Rhythm B at 90/minute; and Rhythm C at 72/minute. Responses of the subjects ($N=23$) to each one of these frequencies were tested against their responses to the other frequencies and also against their response to a control period designated as Silence. Activity levels and changes in these levels were measured by means of a simple rating scale developed for the purpose. The measurements of pulse rate and of general motor activity level were made by a battery of nine observers, before and after each experimental and control session. An experimental design was employed which extended over the mornings and afternoons of five sequential days.

The mean pulse rate was significantly faster under the Combined Rhythms than under Silence. There was no significant evidence of a differential effect exerted upon the pulse rate by the respective experimental Rhythm conditions. The general activity level showed a marked rise under the influence of Rhythm, in decided contrast to the absence of change during the control Silence periods. No one of the rhythmic frequencies was more effective than the others in influencing the degree of change in activity level.

Ratings of behavioural ward adjustment were made for each of the subject patients. Rank order correlations were computed between pulse rate under the several experimental and control conditions and level of behavioural adjustment. In no instance, neither under Silence nor Rhythm, was there a significant correlation between pulse rate change and behavioural adjustment level. In the case of the rank order correlations computed between the level of behavioural adjustment and the increase in general motor activity during Rhythm, statistically significant correlations were obtained.

Familiarization with the drumbeats during the course of the experiment had no effect upon responsiveness of pulse rate or of general motor activity.

It is concluded that chronic deteriorated schizophrenics do respond significantly to rhythmic drumbeat stimuli with an increase in pulse rate and in general motor activity. Within the limits of the pulse frequencies used in this experiment there is no evidence of a differential effect from different tempos. Motor responsiveness to the rhythms is correlated with general behavioural adjustment.

The physiological effect of music rhythm upon schizophrenics demands further exploration.

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EXPERIMENTAL METHOD IN CLINICAL PSYCHOLOGICAL PRACTICE*

By

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PERHAPS in no other applied discipline are there to be found so many differences of opinion as exist in clinical psychology today. Clinical psychologists in fact even disagree about the fundamental approach to their own subject. Let us examine briefly what appear to be the major approaches.

As Eysenck (2) has pointed out, many clinical psychologists who work in mental hospitals appear to accept the role of a technician. They accept both the conceptual system and the practical methods of the psychiatrists they work for. Essentially they seek to answer questions of diagnosis and aetiology by means of tests which have grown out of psychiatric theory and which have usually been validated against psychiatric opinion. The implications of the test results are assessed by the doctor in the light of his own theoretical system, and the treatment carried out is not based on any psychological theories or experiments. The clinical psychologist, particularly in America, may assist in this treatment also, giving psychotherapy under medical direction, so that his role becomes completely subordinate. The psychologist in fact is an assistant psychiatrist with a little special knowledge of certain tests, which he gives as directed. This situation would be justifiable, if not very satisfying, if psychiatric theories were well established, and psychiatric treatments of proven efficacy. Unfortunately this is not the case.

The alternative role for the clinical psychologist is that of an independent scientist, who makes his own unique contribution. This relationship is more like that between the physician and the biochemist. The biochemist is an independent scientist who formulates and investigates theories in biochemistry often fundamental to psychiatry. He applies his scientific knowledge and method to answer specific questions put to him by the physician. He is not told what tests and procedures to adopt, nor do his methods and theories assume the *a priori* validity of those of the psychiatrist.

This approach sounds more acceptable to many psychologists as they argue that the basic training of the psychologist is in science and psychology, not in clinical medicine or therapy. There is one basic difficulty, however. In our analogy, the biochemist while pursuing fundamental research of his own, can also *apply* his science to answer routinely specific questions put to him by the doctor. His immediate practical value to the hospital is that of an *applied* scientist. If the clinical psychologist is to be an applied scientist, he also must have a science to apply. Some objective psychologists appear to believe that there exists a science of abnormal psychology which can be applied routinely. They often administer a routine battery of tests to all patients entering the hospital. Whether such batteries have much practical value is doubtful however. In fact it is doubtful whether there *exists* what might be called a science of abnormal psychology at the present time which we can apply routinely. We have

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on the one hand the beginnings of a science of general psychology, and on the other a small body of empirical psychiatric facts and a larger body of untested psychiatric theories. However, we have no general scientifically tested theory about the causes and treatment of psychological abnormalities.

This, then, appears to be the basic dilemma of the clinical psychologist. If he is to accept the role of a scientist he finds himself in the practical situation with no science to apply. It is not so surprising perhaps that some clinical psychologists have slipped into the role of an assistant psychiatrist.

Some clinical psychologists who are often very aware of the present shortcomings of the science of abnormal psychology, and are also dubious about the practical value of batteries of objective tests applied routinely, have tended to reject the scientific role completely. They practice what might be called the *art* of clinical psychology. Some frame of reference such as psychoanalysis is adopted. Using interview procedures, projective tests and sometimes even analysis of handwriting, they attempt to "understand" the patient. Their conclusions and recommendations are based fundamentally on a subjective personal interpretation of the patient's behaviour, making use of general rules of procedure like artists in other fields. The main objection to this approach is that it is not possible to assess the validity of the conclusions arrived at until after they have been acted on, and the results observed. This method of work also depends on the capacity of the individual doing it and there would seem to be no way of establishing this without conducting an elaborate investigation into the validity of each individual psychologist as an oracle. Even if such an individual were shown to be a fine judge of character, we would still not necessarily know how he did it.

Some psychologists who would reject the idea of making clinical psychology an art rather than a science, would maintain that we should stick to fundamental research and not try to do any applied work at present.

Of course we must admit that such basic research is essential. Nevertheless, it does not necessarily follow that we can do no practical work. In fact the dilemma which was suggested is perhaps not a dilemma at all. Science has two aspects. It is a fairly well established body of tested theory and fact, but it is also a *method* of answering questions. If clinical psychology lacks this body of knowledge it can at least apply scientific method to individual problems. We may have no general explanation of a certain psychological abnormality in all people. However, in the course of clinical work an individual might be referred to us with such an abnormality. The abnormality can be investigated scientifically in its own right. We can first seek to define and describe the symptoms exactly, then put forward hypotheses to explain them, make deductions from these and test them experimentally on the individual. We must discard our hypotheses if the predictions are not fulfilled, and erect new ones in their places, testing these in turn. Eventually we may arrive at an explanation of this individual's specific abnormality and, therefore, be able to bring it under experimental control. If we can do this we may be able to cure the abnormality. This does not necessarily account for this abnormality in all people. However, it has not been our purpose to do this. The explanation may of course have implications for more general theories. An experimental investigation of a single case, if successful, will almost inevitably lead to hypotheses with general implications which would merit research in their own right. In this way clinical work can go hand in hand with research, research problems being suggested by experiments on individual cases, and the investigation of individual cases making use of general research findings.

This general approach to clinical psychology which has been pioneered by M. B. Shapiro (4) is the application of the experimental method to the study of a single case. While it is easy to describe the method in broad outline, it is perhaps not so easy to put it into practice. This is hardly surprising. If the problems of abnormal psychology were *easily* susceptible to experimental enquiry the field would undoubtedly be more advanced than it is. The practising psychologist will find that he has too many patients to conduct elaborate experiments on each one. An experimental investigation of one case takes a very long time. Some of our studies have, in fact, taken as long as a year with some testing being done at least every week. Even at the end we have often found that we have not answered the question we set out to answer. In spite of these difficulties, however, this approach is justifiable for the following reasons. First, if the criticisms of the alternative methods are valid, it is the only thing we *can* do. It is better to accept the fact that we must see fewer patients and spend longer with each, than to resort to simple procedures which have no practical value anyway. Secondly, this method forces one to formulate the problems presented by an individual much more clearly, and this in itself is valuable even though little actual research is done on the individual. Thirdly, even when experimental investigations fail in their ultimate aim, either through lack of time or because of the difficulty of the problem itself, they nevertheless succeed in obtaining some useful facts about the patients which can be used in their rehabilitation or treatment. Furthermore, these facts would not have been discovered if some different approach had been adopted. In the course of disproving a plausible theory about a patient's illness, striking and surprising information often emerges which one would not have been able to predict.

Perhaps the greatest single value of this approach is that we have to formulate clearly the problems presented by the psychiatrist. It is easy to accept glib explanations of abnormal behaviour if they do not have to be tested. If, however, one is forced to formulate these explanations in such a way that they can be *disproved* by an experiment on the individual, one finds that many explanations are not explanations at all because nothing follows from them. Absolutely no predictions can be made from them. Similarly, many apparent problems presented by the psychiatrist turn out to be meaningless or at least very different when one is forced to design experiments to look into them.

The following are two examples of problems which psychiatrists have actually put. On the face of them they seem very similar, and they are typical of a large group of questions clinical psychologists try to answer.

The first request is as follows: "I should like you to give this patient a Rorschach to aid me in making a differential diagnosis between obsessional neurosis and schizophrenia." The second request is: "I should like you to give this patient a Rorschach to help decide whether he is a depressive or an organic case; that is, a pre-senile dementia."

Both are questions of differential diagnosis. Many psychologists would not hesitate. Both patients would be given the Rorschach and a diagnostic label would be suggested for both on the basis of the test. What the psychiatrist would do with this information would be left to him. A more critical psychologist might object to being instructed to give this particular test. He would perhaps feel that there is no evidence that the Rorschach can discriminate between the diagnostic groups in question. He might, therefore, suggest a better validated test giving an answer which quoted a probability in connection with

either diagnosis. For example, for Case 1 he might say: "In a recent study, only 5 per cent. of schizophrenics obtained so low a score on this test, whereas 50 per cent. of an obsessional group got similar scores. I think, therefore, it is more probable that the patient is an obsessional neurotic than a schizophrenic." It might be felt that the second type of answer is better than the first. However, again it is left to the psychiatrist how to interpret these results.

Neither of these psychologists has performed an experiment, however. Neither has formulated a testable hypothesis about anything. In fact, neither has tried to find out exactly what abnormality he is seeking to explain. If we are to experiment on the patients we must clarify our ideas considerably. Let us take the first question: "Is this an obsessional neurosis or schizophrenia?" We must first discover what the psychiatrist means by the terms "obsessional neurosis" and "schizophrenia", as their meaning tends to differ from psychiatrist to psychiatrist. Secondly, the question may be meaningless anyway. There is no reason why the patient could not be both obsessional *and* schizophrenic, if these terms refer to an extreme position on certain unrelated personality dimensions. Therefore, we must go back to the psychiatrist to find out *why* he is asking the question. After discussion, when the problem is shorn of its psychiatric terminology it might really be: "Why does this patient, an intelligent man, believe that all his work-mates are spreading false stories about him and how can I cure him of this idea and its concomitant emotional disturbance." Now that we have the question we can at least try to answer it. The psychiatrist himself may well have theories about it which we can test. We might discover when these were examined that they were essentially untestable; that is, nothing could be predicted from them. The psychiatrist may believe that psychotherapy might help. The psychologist could then examine the evidence relating to the success of psychotherapy for people with these specific symptoms. The evidence might suggest that this is not a good idea. In the end the psychologist may find that he himself is forced to suggest hypotheses to account for the symptoms, and to test them experimentally. A great number of possible explanations might be formulated and tested by experiment. The investigation would clearly be a long one and success could not be guaranteed. In fact the problem is very much more difficult than it at first seemed. The psychologist is faced with a piece of behaviour which is clearly abnormal and must find some way of explaining it with the hope of bringing it under experimental control, so that it can be modified permanently. A simple routine test will probably be quite valueless in suggesting how this might be done.

The second example, if examined, might be entirely different. The psychologist was asked to help decide between a diagnosis of depression and an organic state, perhaps pre-senile dementia. If the psychologist does *not* give a simple diagnostic test immediately he must re-examine the exact nature of the problem. After careful discussion with the doctor the following picture might emerge: This man of 45 is clearly very depressed and it is assumed that E.C.T. is a cure for the specific symptom of depression. However, on clinical examination, the patient gave signs of some memory impairment and it was thought that this might be due to some brain lesion such as occurs in pre-senile dementia. If this were the case, E.C.T. might make the memory impairment worse and it would not be advisable. The psychologist's course of action is now quite straightforward. At present the *only* evidence for a brain lesion is a clinical impression of impaired memory. The psychologist can find out exactly what questions seemed to elicit memory impairment. He can repeat these under controlled conditions. He can test a control group of patients of this age with

no brain injury. He might prefer to use a standard test of memory for people of this age if it seemed appropriate. However, the problem is clear. Essentially he is trying to decide whether the patient has a specific disability. He is trying to discover whether the patient's memory is abnormal compared with people of this age. If it is *not*, the psychiatrist has been misled and brain damage need not be postulated. The problem is now merely whether E.C.T. will alleviate the depression. The point about this method of working is that instead of tackling a rather vague diagnostic problem, the suspected abnormality of the patient, i.e. possible memory impairment, is tackled directly by experimental methods.

It is not being suggested that many sensible clinical psychologists would not have gone closely into both these cases and discovered most of the relevant facts. It *is* being suggested that the experimental approach by its very nature forces one to cut through vague terminology which can obscure the issue and, therefore, to discover the essence of the problem before any testing is done at all. In this way problems can be assessed more easily. Some are clearly very difficult to solve but some at least can be answered fairly easily. In a busy hospital, if we are to be of real assistance to the greatest number of cases, we must concentrate mainly on these simple questions. It is useful to *design* an experimental enquiry as a method of assessing which cases the psychologist should tackle even if it turns out to be too difficult or time-consuming to do.

Clinical psychological investigations can be divided into three types. The first type, usually the simplest, merely attempts to describe the patient in terms of some well-known and more or less clearly defined variable, for example in the assessment of intelligence. Even here it is easy to slip into a familiar routine and prescribe for instance the Wechsler for every case, since it is a fairly well standardized and reliable test. Again, however, this is not an experimental approach. Our investigations, to be experimental, must try to answer some particular question. We must find out *why* the doctor wants an estimate of the patient's intelligence before we can start a sensible enquiry. Sometimes the doctor only wants to discover whether the patient can understand therapeutic interviews. Sometimes he has a more specific aim. He may, for example, wish to advise the patient whether he should return to university. Often we find that the test is used to predict some future behaviour. We must find out what particular intellectual factors are relevant and measure these. If we are to advise whether the patient should return to university we must give tests of those aptitudes which are known to be relevant to the particular course the patient will take. Our purpose is not to compare his intelligence with people in general (as we would do from the Wechsler) but to compare his intelligence and relevant aptitudes with those of a very specific university population. When we have made our measurement, however, we must also take into account the *errors* of measurement and errors of prediction of the tests we are using. We must be able to assess these in making our predictions and so we must make use of tests for which there are adequate data. There can be no measurement or prediction in science without error, and the clinical psychologist must have the statistical means for taking this into account.

Sometimes of course, we wish to describe the patient's intellectual capacities, not to predict future behaviour, but to find out whether some decline has occurred. If we lack pre-illness measures, at least we can use tests for which there are data concerning individuals of this particular education and occupational training, so as to determine how he now compares with a population of which he was formerly clearly a member.

A second group of investigations involves determining the *abnormality* of a certain function. For example, the psychiatrist may wish to know whether an elderly patient's memory is bad enough to be regarded as a symptom. Such questions in general involve first of all measuring the function, and then comparing the individual's score with those obtained in normal people of the same age, sex and intelligence. If the score falls outside this distribution it is clearly "abnormal". Sometimes we have standardized tests available with norms which enable us to make this judgment. Often, however, we do not. We do not, for example, have a test of long-term retention which has been standardized on old people, although tests are available which provide some data for young people. In order to answer the question, we must ourselves test a sample of normal old people of the same intelligence and age and compare our patient's score with these. If no standard test exists at all, we might make one up and collect the necessary control data for it. This type of problem is not difficult although it may be time-consuming.

In any such investigation we are again faced with the problem of errors of measurement. If we use tests for assessing postulated abnormalities for which there are no adequate data, or if we make up tests ourselves, we cannot be sure about the *reliability* of our findings. Again we can only fall back on the experimental approach. If the abnormality really exists it should manifest itself in numerous situations. If we predict that the patient will fail in a series of rather different types of tasks and not in others, and all our predictions are confirmed, the reliability of our measures is confirmed and the hypothesis supported. Purely descriptive investigations of this sort again can be very time-consuming. An example is an investigation carried out on a girl of 21 who was a known post-encephalitic. She had a very severe memory disorder, the nature of which was investigated experimentally. It was shown during a long course of experiments that her failure to recall material was due to her inability to *learn* it in the first place. An interesting discrepancy was observed. Her ability to learn meaningful material (i.e. words or pictures) by the paired associate method, was grossly impaired, while her ability to learn meaningless material (i.e. nonsense syllables or nonsense drawings) was normal. To arrive at the conclusions about the abnormality of her learning, control data were used for some standardized research tests and a control subject was tested for part of the experiments. To arrive at the generalization about the discrepancy between meaningful and meaningless material, however, a long series of learning experiments were conducted. It had to be established that the observed differences in learning rate were not due to such factors as the differences between recall and recognition, the sensory modality involved, the type of material, and so on. The results showed a consistent difference between meaningless and meaningful material, ruling out the possibility that the initial result was due merely to error of measurement.

The third type of clinical investigation is concerned with the *cause* of an established abnormality. These investigations are the most difficult and not all that we have done have been successful. In setting up explanatory hypotheses in a clinical investigation the same rules should apply as apply in ordinary scientific research. In many cases it is possible to think of several hypotheses which can account for the known facts and thus seem equally likely. It is difficult to know which to test first. Other things being equal, the best hypothesis is that which yields the greatest number of predictions. An hypothesis from which *nothing* follows except the facts it seeks to explain, is valueless. This seems to be the commonest fault in much psychiatric theorizing.

Let us consider a concrete example of these two sorts of theories. A boy who came to the Children's Department had two main problems (1). First of all he was emotionally and behaviourally disturbed, particularly at school. Secondly, although 10 years of age, he was completely unable to read despite considerable special tuition of the usual kind. As his symptoms were improving, the investigation concentrated primarily on his reading difficulty which still remained. Preliminary testing demonstrated that his general intelligence was low average, so that this could not account for his disability. He was not more backward in other subjects than one would expect, so that the disability appeared relatively specific. He did not have any sensory defects (deafness or poor eyesight) which could account for it. In the end it would have been possible to formulate two hypotheses: (1) This was a case of congenital word blindness, and (2) This was a boy who was abnormally bad at associating one perception with another: e.g. printed patterns and spoken words.

It was decided to *ignore* the first (in fact it was not seriously considered by the investigators), and test the second hypothesis. The hypothesis of congenital word blindness was quite tenable. It accounted for all the facts—the specificity of the backwardness, the lack of peripheral sensory defects, and the complete inability to learn. However, it had few implications. Very little else could be *predicted* from it. Since congenital word blindness is said to be due to damage in a specific cortical area, it might have been predicted that tests of *brain* damage would yield positive results. However, nothing *else* would follow from the theory even if this prediction were verified. True this formulation has a pessimistic tone, suggesting that treatment would be difficult or impossible, but even this is not a clear prediction, as other congenital defects (e.g. deformed feet) can be treated and cured. The word congenital does not mean unalterable. In fact *nothing* follows from this hypothesis. It is relatively sterile.

The other hypothesis has a number of implications, however. If true, it should affect learning in general. A series of experiments were started to determine whether or not the patient *did* have abnormal difficulty in forming connections between sensory modalities. It was found that indeed he did. By comparison with a control group of readers, it was shown that he was abnormally bad at forming connections within the auditory modality, within the kinaesthetic modality, and between these modalities. He was *normal* at forming connections within the visual modality. It was also found that he was *ultimately* able to learn connections in *any* modality if given enough repetition. That is, he had no defects of retention. It was concluded that these learning defects could account for his failure to learn to read by normal methods. The special coaching programme which was developed for him emphasized the formation of visual associations and repetition. He ultimately learned to read. The first hypothesis, that of congenital word blindness, was never tested. It may well be correct. However, as an hypothesis, whether correct or not, it is virtually useless as so little follows from it. It is not really necessary.

Most of the examples I have quoted were especially time-consuming as they involved the testing of a control group also. This is clearly needed if we are to discover whether or not a function is to be regarded as *abnormal*. However, a control group is not always needed in an explanatory investigation. If we accept the abnormality of a symptom, the subject can be used as his *own* control. That is we seek those experimental situations which improve the patient's *own* performance. Thus he acts as his own control and provided we take care to rule out practice effects, this can be adequate. We still do not know of course

whether a control group would do the same under similar circumstances, but if our treatment is successful the question is irrelevant for that case.

SUMMARY

A general experimental approach to clinical psychological practice has been outlined and it has been suggested that experimental method in clinical psychology is both possible and useful in solving some of the problems in a very difficult field.

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A SHORT SCALE FOR RATING "ACTIVITY-WITHDRAWAL" IN SCHIZOPHRENICS

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EXPERIMENTAL work on the performance of schizophrenic patients indicated that different patterns of response were to be found among patients who could be considered subjectively as withdrawn and inert on the one hand, and sociable and active on the other. This subjective classification appeared to cut across diagnosis within the schizophrenic group.

In order to make this observed classification more objective, the literature on rating scales of psychiatric patients was considered to find items which appeared to be relevant to the subjectively observed classification described above.

Finally, nine items were abstracted from the M.S.R.P.P. Scale devised by Lorr *et al.* (1), and a further item was constructed to measure the apparent friendliness of the patient. All items were modified to be scored on a five point basis if they were not already so scored. The 10 items are shown in the appendix.

One hundred schizophrenic patients, 66 men and 34 women, were rated independently on the scale by two charge nurses in the case of the men, and two ward sisters in the case of the women. The mean age of the men was 40.9 S.D. 6.5 years, and that of the women 45.5 S.D. 9.19 years. The men had been in hospital for a mean period of 8.2 S.D. 7.3 years, and the women 6.4 S.D. 5.1 years.

The 10 scale items with sex and age as additional variables, were inter-correlated and a centroid factor analysis carried out. Three factors significant by McNemar's (2) criterion, were extracted. The loadings obtained after rotation are shown in Table I. Short definitions of items are given to facilitate reference.

TABLE I
Rotated Factor Loadings for Ten Scale Items, Sex and Age

Variable Item	I	II	III	Direction of Scoring
1	.42	.54	.00	Restless
2	.90	-.06	-.05	Overtalkative
3	.56	.70	.00	Overactive
4	.80	-.19	.03	Many friends
5	.90	.12	.04	Loud speech
6	.76	-.14	.06	Many interests
7	.91	-.03	-.02	Much conversation
8	.48	.66	-.06	Hurried actions
9	.89	-.11	.09	Likes company
10	.91	.02	.01	Talks too much
Sex	.44	-.32	.36	Female
Age	.19	-.16	.34	Old

It is seen that Factor I has positive loadings on all the items, while Factor II has its highest loadings on items 1, 3 and 8, which are concerned with activity

rather than with sociability and talkativeness. The third factor is a doublet loaded on sex and age.

If a vector is extended through a cluster of activity items, 1, 3 and 8, it is found that the correlation between this vector and the vector representing Factor I is of the order of $\cdot 70$. It is probably therefore not of practical importance to divide up the items into two groups for separate use.

The inter-rater reliability for the 10 items is $\cdot 79$ for all raters, while that for male raters rating male patients is $\cdot 83$, and that for female raters rating female patients is $\cdot 66$.

Before this analysis was completed a shortened version of the scale omitting items 1, 3, 5 and 8 was used as a criterion of "sociability-withdrawal" in two studies (3, 4), where it was found to distinguish between groups of schizophrenic patients showing, or not showing, a facilitatory effect of extraneous sound or light on simple R.T. The effectiveness of the scale as a research criterion is shown by its usefulness in these two studies; further work to determine its standing with respect to other variables is to be undertaken.

APPENDIX

Items in Activity Withdrawal Scale

1. (42)* How much does he move around? Does he sit all day unless pushed to follow routine? Or, is he usually walking around or restlessly moving some part of his body?

1	2	3	4	5
Usually motionless	Underactive	Moves about as appropriate	Restless	Almost constantly moving

2. (8)* Compared to the average person, how taciturn or talkative is he? Judge only the amount of speech; do not consider its relevance nor whether it was spontaneous or elicited by questioning.

1	2	3	4	5
Mute throughout interview	Distinctly less	As talkative as average	Distinctly more	Conspicuously overtalkative

3. (46)* Does he usually give the appearance of being tired and worn out or lively and energetic as compared to others?

1	2	3	4	5
Almost completely worn out	Tired	As lively as most	Livelier and more energetic	Overactive and abnormally energetic

4. Does he have friends in the ward?

1	2	3	4	5
None at all	Acknowledges one or two other patients	Has one or two fairly regular friends	Is normally friendly	Makes friends with as many people as he can

5. (20)* Compared to others, how loud or intense is his speech? Is it barely audible or is it loud and (or) intense?

1	2	3	4	5
Almost inaudible	Distinctly less audible or less intense	As loud as average	Distinctly louder or more intense	Shouts or yells

* Numbers in brackets refer to numbers of items in M.S.R.P.P. Scale (Lorr *et al.* (1)).

APPENDIX—continued.

6. (57)* How much interest does he show in the things going on around him? Which of the following does he do: (a) listen to the radio or watch television; (b) play games; (c) read newspapers or magazines; (d) go to dances or socials; (e) talk about ward happenings, sport or news events; (f) write letters; (g) go to parties?

1	2	3	4	5
Interested in nothing, just sits	Any one	Any two or three	Any 4 or 5	All 6 or more

7. (56)* How often does he speak to others? Does he ask for things, say hallo, ask questions, make comments, or otherwise start a back and forth conversation?

1	2	3	4	5
Never	Occasionally	Less often than the ordinary person	As often as the ordinary person	Nearly always speaking to someone

8. (71)* Compared with others are his reactions (walking, talking, gestures) slower and more deliberate, or, do they appear faster or hurried?

1	2	3	4	5
Markedly slower	A little slower	As fast as average	A little faster	Conspicuously faster or hurried

9. (58)* Does he stay by himself and avoid others, or does he like being with people?

1	2	3	4	5
Always stays by himself. Ignores everyone	Usually by himself. Mixes sometimes	About as much alone as with others	Usually in company with others	Always in company with others

10. (52)* Typically how much does he talk if spoken to?

1	2	3	4	5
Does not answer	Only 3 or 4 words	Less than the ordinary person	As much as the ordinary person	Hard to stop

* Numbers in brackets refer to numbers of items in M.S.R.P.P. Scale (Lorr *et al.* (1)).

ACKNOWLEDGMENT

This work was carried out at Springfield Hospital, London, by kind permission of the Medical Superintendent, Dr. H. C. Beccle.

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COMBINED TREATMENT WITH CHLORPROMAZINE AND RESERPINE

By

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AN ATTEMPT AT QUANTITATIVE ASSESSMENTS IN CHRONIC SCHIZOPHRENIC PATIENTS

THE frequency with which new drugs become available to the psychiatrist emphasizes the need for quantitative evaluation of their therapeutic effect. Comparable statements, scales of measurement and base-lines are required (Ref. 1-6).

PART I

THE PARKSIDE BEHAVIOUR RATING SCALE (PBRs)

This paper reports an attempt to meet such a demand by developing a rating scale for hospital use by nurses and doctors. Other psychiatric rating scales (Lucero and Meyer, 1951; Malamud and Sands, 1942; Shatin and Freed, 1955) were examined in detail and tried, but found unsuitable for our purpose, which was the fairly quick, reliable and comprehensive measurement of changes produced in chronic schizophrenics by the combined use of chlorpromazine and reserpine. None of the above mentioned rating scales comes from this country, and none of them apparently has been used in the evaluation of the effect of drugs or other forms of treatment. Some contain too many items (Malamud and Sands, Lucero and Meyer); others are of the present-absent type (ABRS). Acknowledgment must be made to the designers of the Albany Behaviour Rating Scale (Shatin and Freed, 1955). The general traits chosen by them have stimulated the design presented here even if the PBRs otherwise bears no resemblance to the ABRS.

Our scale consists of a 5-point scale for each of 6 traits: A. Self Care. B. Orientation. C. Communication and Socialization. D. Psychotic Behaviour. E. Co-operation and Occupation. F. Mood and Reaction to Environment.

In each of the traits several items relevant to them have been grouped together and graded.

The lowest rating a patient may obtain is 6 points (grade I in all 6 traits); the highest is 30 points (grade V in all 6 traits). It was found that a nurse, once well conversant with the PBRs and with the patients to be measured, will take one minute approximately for the measurement of all six traits. From the results obtained with this rating scale both in testing its reliability and later in the trial using it in practice it can be claimed with some confidence that it is workable. Our nursing staff like using it, as can be seen from such remarks as: "we wish we could report on our patients in plain figures like these."

It can be used also under the handicap of a great shortage of medical and nursing staff; the conditions under which it was designed and worked are certainly not comparable with those in normally staffed hospitals, not to speak of research units. It is hoped that the six general traits of the scale cover the

main overt aspects of the psychotic personality; also that the faults of other designs, which lose themselves in individual features of unequal weight making the scale difficult and clumsy to work and to evaluate changes statistically, have been avoided.

It may well be that future forms of treatment will affect some aspects only of the behaviour of psychotics. In our trial we found that improvement progresses approximately *pari passu* in all aspects. Orientation (Trait B) is to a large extent dependent on Communication and Socialization (Trait C). However, after careful consideration it was decided to retain this item as it offers such a good opportunity for the psychiatrist to check the rating. As either increase or decrease of psychomotor activity may constitute an abnormality in schizophrenics it was not possible to provide for inclusion of this item in a scale allowing for measurement in one direction only. We have tried to include it, rather loosely, in the term "psychotic behaviour". See rating scale at the end of the article.

THE RELIABILITY OF THE PBRS

This rating scale was tested on ten female patients by three nurses on two occasions (interval one week), i.e. six measurements on each patient, the patients having been chosen in alphabetical order with the sole proviso that they keep static enough to allow such an interval.

Next it was tested on ten male patients (patients from wards not under the author's care) by two male nurses on two occasions with a 4-day interval, i.e. 4 measurements on each patient. Correlation-Coefficients were calculated between the nurses' free unaided ranking and the values of the ranked PBRS figures. In addition the measurements of various traits of the PBRS on these cases were submitted to the same procedure. The reliability of the PBRS is obvious from the figures as expressed in the significance tests: i.e. this rating scale gives the same results in different hands and also on retesting by the same observers. The Ranking Order Correlation Coefficients range between 0.93 and 0.99, *t*-test results are between 6.99 and 18.07, the significance at 8 DF being $P < 0.001$. See Table I.

The mean group rating of each of 3 raters was almost identical and equivalent to the mid-point of each range. There were no significant differences between ratings on the same occasions and almost none between occasions. Each of the five raters drew ratings from the major part of the scale. For each patient the mean rating based on the five raters did not change significantly from occasion to occasion. Also none of the sub-groups is significantly more unreliable than the other.

Considering the individual traits: for each trait, the major portion of the scale was again used by the raters. The widest range was on Trait A with 3.34 points and the smallest in Trait D with 2.67 points. It is clear also that there is a high correlation between traits (*R* between 0.78 and 0.98, *t* between 3.51 and 12.68, i.e. $P < 0.01$). See Table II.

VALIDITY

From the viewpoint of validity one can at least say that the stability reflected in the ratings is supported by the general opinion that none of the patients had changed significantly during the period between the two ratings.

The items are drawn from clinical psychiatry and the changes produced by the treatment in the treatment-group (and not significantly so in the control-group), as noticed by the medical officer, are reflected in the changes of the measurements with the PBRS carried out by the nurses.

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PART II

THE DOUBLY CONTROLLED TRIAL
THE PATIENT MATERIAL

Sixty female patients suffering from chronic schizophrenia were chosen in alphabetical order from one ward. Thirty of them acted as controls. The ward is one of the two most disturbed wards in the hospital. The treatment group of 30 consisted of eleven predominantly hebephrenic, seven paranoid, nine simple schizophrenic and three catatonic patients. Their age range was 20-68 years with a mean of 42 years and a standard deviation of 12.95 years. The duration of the psychosis ranged from 3-29 years with a mean of 12 years and a standard deviation of 7.64 years. The depth of the psychosis as measured with the PBRs was:

Points of PBRs	6-8	9-11	12-14	15-17	18-20	21-23	24-26	27-29
Before treatment								
1.12.55 ..	10	4	7	4	5			
(30 patients)								
After treatment								
1.2.56 ..	1	4	5	3	8	5	4	
(30 patients)								
After placebo								
1.4.56 ..	3	5	4	2	5	7	3	1
(30 patients)								

Figures indicate number of patients in each group.

The control group of 30 consisted of 12 predominantly hebephrenic, 8 paranoid, 8 simple schizophrenic and 2 catatonic patients. Their age range was 33 to 74 years with a mean of 50 years and a standard deviation of 10.97 years. The duration of the psychosis ranges from 3 to 53 years with a mean of 20 and a standard deviation of 11.34 years. The depth of the psychosis as measured with the PBRs is:

Points of PBRs	6-8	9-11	12-14	15-17	18-20	21-23	24-26	27-29
1.12.55 ..	8	5	1	5	3	6	1	1
(30 patients)								
1.2.56 ..	6	7	2	3	5	3	3	1
(30 patients)								
1.4.56 ..	7	5	2	4	5	3	3	1
(30 patients)								

TWOFOOLD CONTROL

The aim was to change one factor only: the treatment with tablets, other factors such as intensity of mental nursing, habit-training and occupational therapy remaining unchanged.

The thirty control cases were assessed side by side with the treatment cases with the PBRS on 3 dates:

1. At the start of the treatment, on 1 December, 1955.
2. After two months treatment, on 1 February, 1956.
3. After gradual withdrawal of the drugs and replacement by placebo over six weeks, and 3 weeks on placebo only, on 1 April, 1956.

The assessing nurses did not know when and how placebo was substituted. As far as we know this is the first study dealing with a quantitative assessment of the combination of the two drugs, and that such a gradual withdrawal has been carried out, followed by the re-assessment. The gradual withdrawal seems more in keeping with clinical practice where such a procedure would normally be adopted (refs. 1, 2, 4, 6, 8, 10).

PREVIOUS TRIALS WITH THIS COMBINATION

The effectiveness of this treatment has never been doubted, as can be seen from a number of papers (see refs. No. 3, 5, 7 and 9). It is very difficult to compare them owing to the lack of quantitative data employed, and the absence of agreed measurable standards. It is difficult to say whether the combination really is more effective than either drug alone, a rough assessment only having been made by Pollack, who concluded that chlorpromazine was superior. He differentiates between improvement of the psychosis as opposed to that of behaviour. This interesting approach however does not separate the acute from the chronic patients nor has any indication been given of the basic state of his patients regarding the depth of the psychosis which must necessarily differ widely.

In some papers the dosage scheme is too complicated for use in the average mental hospital and the patient material in other articles represents almost the whole range of psychiatric illness.

DOSAGE

We started with 25 mg. of chlorpromazine + 0.25 mg. of reserpine and increased this every second day by the same dosage to a total of 150 mg. of chlorpromazine and 1.5 mg. of reserpine. Eleven patients who did not show any response from that dosage were further increased to 225 mg. of chlorpromazine and 2.25 mg. of reserpine and 8 of these after a further week to 300 mg. and 3 mg. respectively. At first the tablets had to be crushed and given in a drink.

RESULTS

The changes which occurred with the above treatment can best be seen from the shift which is evident from the two diagrams.

All the pre-treatment measurements added together (30 patients) give the figure 350 (448) which after two months' treatment becomes 524 (452) and the regression under the placebo is from 524 to 508 (455). The figures in brackets indicate the (insignificant) changes in the control group.

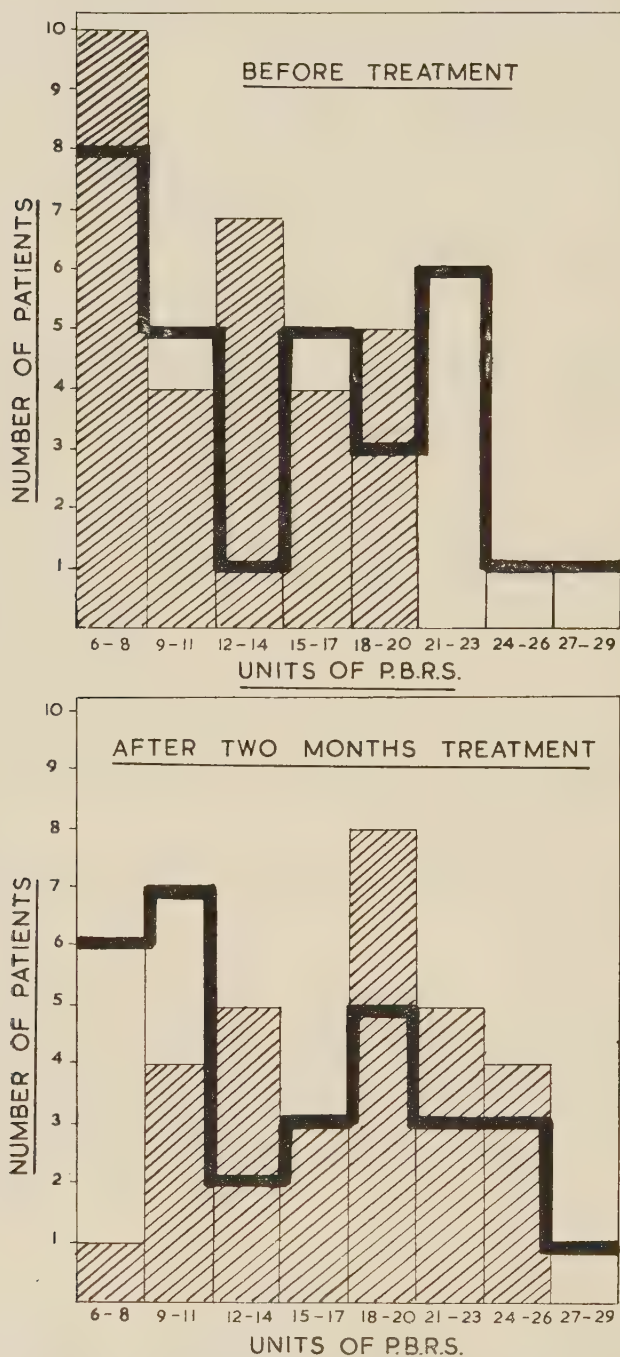


FIG. 1.—State of 30 patients before and after two months treatment with Chlorpromazine and Reserpine as measured with the P.B.R.S. (The black line represents the 30 control cases.)

The improvement of 174 points is distributed with the 6 traits of the rating scale as follows:

Trait ..	A	B	C	D	E	F	
Points ..	34	33	28	25	26	28	=174

The improvement in points per patient:

Number of patients	1	0	5	3	2	3	2	6	2	2	1	2	1
Improvement in points of PBRs	0	1	2	3	4	5	6	7	8	9	10	11	12

The differences have been statistically assessed both in the treatment and control groups for all traits combined and for each trait with the formula:

$$t_{58DF} = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{\frac{\sum (X_1 - \bar{X}_1)^2 + \sum (X_2 - \bar{X}_2)^2}{N_1 + N_2 - 2}}} \times \sqrt{\frac{N_1 \times N_2}{N_1 + N_2}}$$

See Table III.

The differences were found highly significant when comparing the measurements before the beginning of treatment with those after two months' treatment with the combination: $t=4.76$ for the total of all traits, and ranges from 3.62 to 5.13 in the various traits, i.e. it is highly significant at the 0.001 level (3.46 at 0.001). The improvement is still significant after two months on placebo but some regression has occurred (0.01 in one of the traits). The regression under the placebo is not significant even at the 0.5 level. The impression is however, that if we had waited for another month, or had replaced the drugs suddenly by the placebo instead of tapering them off over six weeks a regression to almost the same level as before treatment would have occurred. There was no statistically significant difference in the control group between either of the three measurements, nor in any of the traits.

In terms of behaviour these changes, as measured with the PBRs, mean that there has been a drastic change in this once very disturbed ward of 89 patients, even if only 30 of them had the treatment. One of the patients (13 years an in-patient) has been discharged and six are in the process of discharge. Three more are now fully employed and seven more will soon be ready for discharge except that there is no home to receive them.

SIDE EFFECTS

Were the same as reported in many other papers; the same applies to the reduction in blood pressure. Several alterations in dosage became necessary, also temporary suspension of one or the other drug.

CONCLUSIONS

1. It is evident that this combination of drugs produces good clinical results in chronic schizophrenics.
2. These results are so significant that they can be demonstrated by a rather coarse method of quantitative assessment, such as the Parkside Behaviour Rating Scale, which has been tested as to its reliability.

3. In view of the absence of comparable data it is not possible to say how much of the improvement is due to either drug, but the impression is that they work synergistically.

A report on a larger series is in preparation, also a study of the effect of heavy reserpinization (up to 15 mg. daily) on the same sort of case material as measured with the PBRS.

SUMMARY

An attempt has been made to measure the depth of the behaviour aspects of chronic schizophrenia and the improvement that has been obtained by the combined administration of chlorpromazine and reserpine.

The Parkside Behaviour Rating Scale was designed for this purpose of quantitative assessment of the changes produced. The trial was doubly controlled. The improvement was found highly significant both clinically and on statistical analysis of the data.

ACKNOWLEDGMENTS

My thanks are due to Prof. E. W. Anderson, Department of Psychiatry, University of Manchester, who suggested the investigation and to Dr. Crowe, Medical Superintendent, for his kind interest in the study and permission to carry it out, also to all my colleagues at Parkside Hospital, especially Dr. Aitken and Dr. Monks. I also wish to express my thanks and appreciation for the help and willing co-operation of Sister Fletcher, Nurses Davy and Abbot on the female side and Charge-Nurse Abrahams and Nurse Pratt on the male side, to the latter also for executing the diagram. I am also indebted to Mr. J. C. Kenna, Lecturer in Clinical Psychology to the Department of Psychiatry, University of Manchester for statistical assistance with the analysis of the results. The placebo tablets were kindly made available by Messrs. Ciba (Reserpine) and Messrs. May & Baker (Chlorpromazine), the library service of the latter having been found most valuable in tracing the references.

Samples of the PBRS can be obtained from the author on request.

TABLE I

Ranking Order Correlation-Coefficients Between Raters and Their Statistical Significance

Ranking Order: Correlation-Coefficients $R = 1 - \frac{6\sum D^2}{N^3 - N}$	Submitted to T-test	
	"t" = $R \times \sqrt{\frac{N-2}{1-R^2}}$	"t"
R		
1. Sister S. own ranking compared with PBRS ranking	0.93	6.99 (5.07 at 0.001 with 8 DF)
2. Nurse D. own ranking compared with PBRS ranking	0.94	7.75
3. Nurse D. ranking of ten treatment cases before treatment and compared with their PBRS ranking	0.85	4.55 (3.36 at 0.01)
4. Nurse A. (F) ranking compared with PBRS ranking	0.96	10.12
5. Charge Nurse A. (M) ranking with his 1st PBRS ranking	0.95	8.60
6. Nurse P. ranking with his 1st PBRS ranking ..	0.92	6.42
7. Nurse P. ranking with his 2nd PBRS ranking (48 hours)	0.99	18.07
8. Nurse P.'s two PBRS rankings correlated ..	0.98	12.68
9. Averages of the first PBRS measurements by 3 nurses ranked and correlated with their 2nd ranked PBRS rating	0.99	36.39

TABLE II
Ranking Order Correlation-Coefficients Between Traits and Their Statistical Significance

	R	"t"
10. Averages of 3 Trait A measurements on 10 patients correlated with Trait B ranking ..	0.94	7.52 (5.04 at 0.001 with 8 DF)
11. Averages of Trait A ranking (6 measurements correlated with Trait B)	0.93	6.99
12. Trait A (6 measurements) correlated with Trait C	0.92	6.42
13. Trait B correlated with Trait E	0.95	8.24
14. Trait A and Trait F	0.78	3.51 (3.36 at 0.01)
15. Trait C and Trait E	0.98	12.68
16. Trait D and Trait F	0.95	8.80

TABLE III
Differences in Ratings Submitted to T-test

$$t_{68DF} = \frac{\bar{X}_1 - \bar{X}_2}{\sqrt{\frac{\sum(X_1 - \bar{X}_1)^2 + \sum(X_2 - \bar{X}_2)^2}{N_1 + N_2 - 2}}} \times \sqrt{\frac{N_1 \times N_2}{N_1 + N_2}}$$

Treatment Group	Before Treatment Compared with After 2 Months' Treatment	Before Treatment Compared with After 2 Months' Treatment and 2 Months' Placebo	After 2 Months' Treatment Compared with After 2 Months' Placebo
Total ..	4.77 0.001 (3.46)	4.16 0.001	0.39 no significance
Trait A ..	5.13	4.43	0.16 no significance
Trait B ..	3.62	2.87 0.01 (2.66)	0.49 no significance
Trait C ..	4.27	3.25	0.40 no significance
Trait D ..	4.29	5.75	0.14 no significance
Trait E ..	3.88	3.23	0.38 no significance
Trait F ..	4.09	3.28	0.54 no significance
Control Group			
Total ..	0.54 no significance	0.14 no significance	0.07 no significance
Trait A ..	0.0 no significance	0.23 no significance	0.22 no significance
Trait B ..	0.37 no significance	0.61 no significance	0.21 no significance
Trait C ..	no significance	no significance	no significance
Trait D ..	no significance	no significance	no significance
Trait E ..	no significance	no significance	no significance
Trait F ..	no significance	no significance	no significance

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THE PARKSIDE BEHAVIOUR RATING SCALE (PBRs)

	1	2	3	4	5
Self-care					
A	a Requiring every form of nursing care. b Most doubly incontinent. c Not caring for appearance, food dribbling on dress. d (tube feeding or) Forced feeding.	Requiring regular supervision. Faecal incontinence at times only, usually urinary incontinence only. Little, or occasionally only, caring for appearance. Has at times to be fed.	Usually attends to own personal habits. Occasional urinary incontinence only. Dress kept fairly clean without regular supervision. Has at times to be urged to eat.	Verbal nursing care only. Good habits. Usually clean and tidy, active interest in clothing. Eats when meal supplied.	Boarder. Spotless habits. Always clean and tidy, wears own underclothing, cares for it herself/himself. No feeding comments.
Orientation (as far as communicated)	Completely disorientated for time, place and identity.	Orientated for place and identity, knows function of staff.	Orientated partially for time (month, year) and knows name of one or more of staff.	Knows day of week, month, year, place and names of most of staff.	Fully orientated, knows all personnel by name, knows length of stay in hospital and names of many patients.
B					
Communication and Socialization	a No spontaneous speech, or rambling only, no answers if spoken to. b No social contact, no spontaneity, no rapport. c No interest in visitors, family or letters.	Speech comprehensible but often rambling, replies to questions often irrelevant. Sometimes spontaneity but not relevant to situation, at times superficial rapport. Withdrawal only broken by interest in others playing part in delusions, fatuous interest in visitors, no interest in letters.	Speech mostly relevant, sensible answers, at times disturbed by thought blocking. Spontaneity, but at times irrelevant to situation, sometimes good rapport but mostly superficial. Shows interest in others even if at times interfering, some relevant contact with visitors, reads and understands letters, and attempts answering them even if not carried through.	Speech relevant in content, at times only some irrelevance. At times conversing spontaneously, behaviour relevant to situation, often good rapport. Social contact with others generally maintained, occasional withdrawals, partakes in recreations and some social activities, good contact with visitors, sometimes writing letters.	Speech relevant, sensible, spontaneous. Free, good rapport. Full social contact with patients and staff (active part in social club), permanent pen relationships.
C					
Psychotic Behaviour	d Complete apathy. a Talks back to auditory hallucinations. b Always severely deluded with hostility and aggression, destructive with clothing, bedding and resistive to attention. c Frequent psychotic outbursts.	Usually apathetic. Has periods of freedom from, or only background hallucinations. Deluded with hostility and aggression at times only, sometimes destructive. Psychotic outbursts are shorter and free intervals occur.	Periods of apathy. Has times when hallucinated but not interfering with other activities. Delusions, but hostility only an under-current, few abortive destructive threats and aggressive attitudes. Long periods of freedom from delusions but behaviour lasting hours only when occurring. Has to be put to rest occasionally for 1 hour.	Has occasional poorly defined auditory hallucinations. Delusions suffered at times but accepted as a matter of fact, partial insight, not destructive. Has undercurrents of fleeing hostility for seconds or minutes only. On ground parole.	No hallucinations. No delusions, or not recognizable except after long search and then has insight or pays no attention. Never has disturbed outbursts.
D	d Requires seclusion or continuous observation. a Completely unco-operative, resisting any interference.	Seclusion occasionally necessary. At times accessible (at examination), at times co-operative, low grade forms of occupational therapy possible (such as P.T.).	Generally co-operative, attends O.T. class regularly; can carry out simple limited jobs.	Co-operative patient usually usefully employed under supervision, passed out from O.T.	On full parole, week-ends home. Active participation in work (sees work himself) some responsibility taken in work.
E					
Co-operation and Occupation					
F	a Mood flat, indifferent, depressed, or psychotic laughter. b No contact with reality, complete withdrawal. No interest in environment.	Mood changes frequently, often depressed sullen. At times fleeting contact with reality, but mostly withdrawn. At times fleeting interest in environment.	Sometimes depression, mood often adequate. At times full contact with reality but usually midway "frozen state". Moderate interest in environment.	Mood shows fleeting depressions only. Usually full contact with reality, at times some remoteness. Usually interested in all events, fleeing "flaps" only.	Mood stable and appropriate. Full contact with reality. Active, full interest in environment.

THE REES-EYSENCK BODY INDEX OF INDIVIDUAL SOMATOTYPES

By

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ANTHROPOMETRY has benefited in the last 20 years from two distinct advances, the use of standardized photography by Sheldon (1940) and the application of factor analysis to physical measurements as initiated by Burt (1944). Tanner in a critical review (1947) compared these two main approaches to interpretation of body type. Later (1954) he defended photoscopic somatotyping against the charge of subjectivity in ratings by stating the degree of consistency obtained by experts, although such consistency is admittedly less common among less expert somatotypists. Factor analysis can claim some theoretical advantages, and certainly greater mathematical exactness than photoscopic typing: but here the question arises as to whether the measurements so far employed for factor analysis yield the best information; statistical treatment by itself, however sophisticated, is no substitute for the best or right choice of measurements.

Rees and Eysenck (1945) factor-analysed the measurements of 200 neurotic soldiers, deriving a body index from their analysis:

$$\text{R.E. Body Index} = \frac{\text{Stature} \times 100}{6 \times \text{Transverse Chest Diameter}}$$

Because of the rival claims of somatotypists and factor analysts, I decided to determine the average Rees-Eysenck Body Indices for as many individual somatotypes as possible from a series of approximately 3,000 young men, including many students at the Universities of Oxford and Birmingham, who had been measured and somatotyped between 1948 and 1956. The results are set forth in Table I and the trends are illustrated by contour lines in Figure 1. It is evident that the highest average values for the R.E. Body Index are found in Sheldon's extreme ectomorphs. Sheldon's ectomorphs and R.E. leptomorphs clearly have a great deal in common. This was not unexpected, for Sheldon's component ectomorphy correlates closely with the ponderal index (height³:cube root of weight) and height also forms the numerator of the R.E. Body Index. As denominators, chest width and height appear in the two indices. Any measurement such as chest width, which tends to increase cross section for a given height, would seem likely also to increase weight: in fact, the leptosomic factor was found to correlate not only positively with height (+0.54) but negatively with both chest width (-0.54) and weight (-0.23), and it is evident that the two Body Indices would be likely to share a common tendency.

What could not be anticipated so easily was that the lowest R.E. indices would be found in ectopenes who were mesomorphic rather than endomorphic. This brings us to the crux of the matter, for chest width, like other traditional anthropometric measurements, is a composite measure embracing fat, bone, muscle and vital organs. It is not easy to see on *a priori* grounds why chest width should possess power to distinguish between these tissues. Yet these tissues have quite different functions, and in any morphological classification.

TABLE I

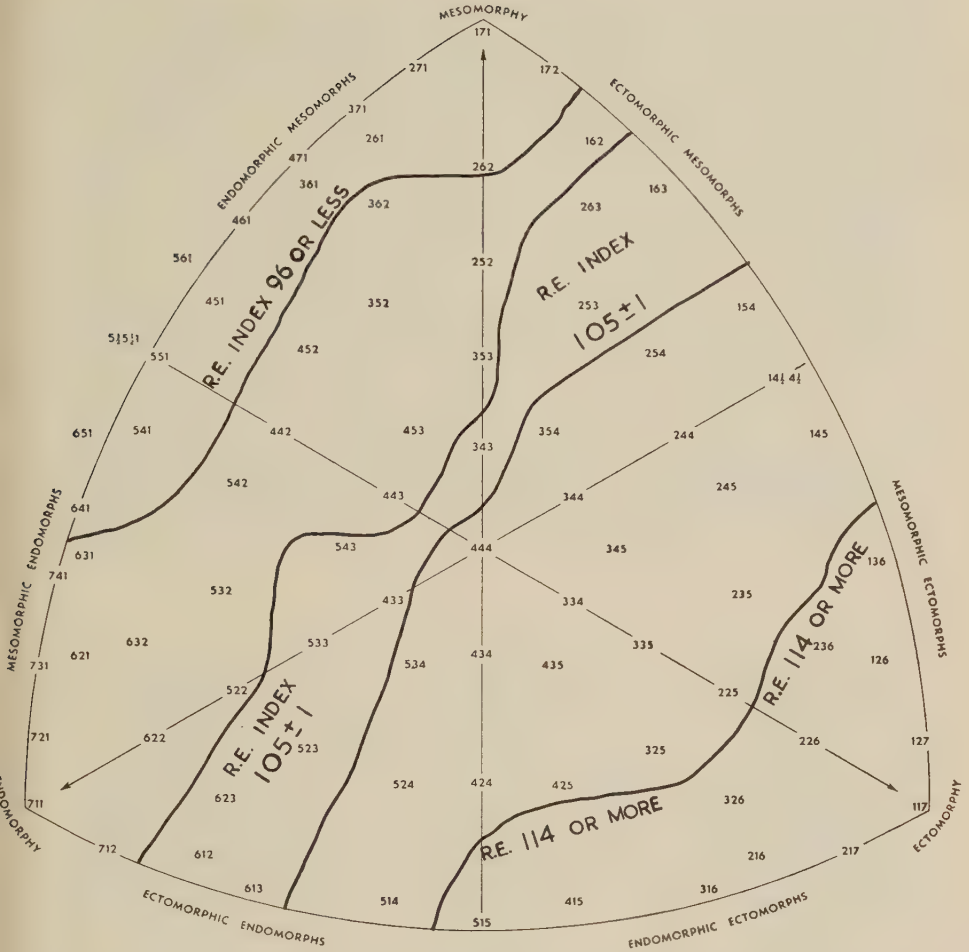
Mean Values of the Rees-Eysenck Body Index for Individual Somatotypes

Somatotype	Rees-Eysenck Body Index	Somatotype	Rees-Eysenck Body Index
117	116	415	115
127	116	424	111
126	114	425	113
136	117	433	105
145	112	434	109
154	108	435	107
162	100	442	101
163	104	443	101
171	95	444	109
172	96	451	94
		452	102
217	116	453	104
216	114	461	92
225	113	462	99
226	116	471	95
227	116		
235	112	514	112
236	115	515	113
244	105	522	101
245	111	523	107
252	104	524	111
253	105	532	103
254	107	533	105
261	96	534	110
262	96	541	95
263	104	542	100
271	94	543	105
272	96	551	98
		552	99
316	114		
325	113	621	100
326	113	622	100
334	112	631	99
335	112	632	100
343	105	641	95
344	107	651	94
345	108		
352	99	731	100
353	102	741	98
354	107		
361	95		
362	99		
371	96		

aimed at achieving high correlation with behaviour it would seem reasonable at least to separate fat and its function from muscle and motive power.

The trends illustrated in Figure 1 show that for each level of R.E. index in more muscular somatotypes there is an endomorphic counterpart with similar index, and that the R.E. index does not distinguish between them. Thus an Olympic long distance athlete of somatotype 145 may have almost the same index (112) as a comparatively fat and feeble man of somatotype 514 or 524 with physique close to that of Froehlich's syndrome. Or, as a further example, a 271 competitor in the "Mr. Universe" physique contest* would have the

* See Appendix.



REES-EYSENCK BODY INDEX CONTOURS OF MEAN SOMATOTYPE VALUES. A SCHEMATIC TWO-DIMENSIONAL PROJECTION OF THE THEORETICAL SPATIAL RELATIONSHIPS AMONG THE KNOWN SOMATOTYPES

FIG. 1.—Illustration of trend in mean values of Rees-Eysenck Body Indices for individual somatotypes.

same index (94), on the average, as an obese man of somatotype 641 who could not compete for fear of becoming a laughing stock. These are major differences in type, unlikely to be confused photoscopically, or indeed by the most casual observer (see Figs. 2 and 3).

It might be concluded that measurements of fat and muscle should be added to the correlation matrices from which factors are derived in future analyses. The question arises, however, as to whether a technique as complicated as factor analysis is appropriate. Would not a series of scores in standard measure for fat, bone and muscle yield more satisfactory information in form far simpler than the product of a regression equation? This has been attempted (Parnell, 1954; Parnell *et al.*, 1955) and the method has been extended (1956)

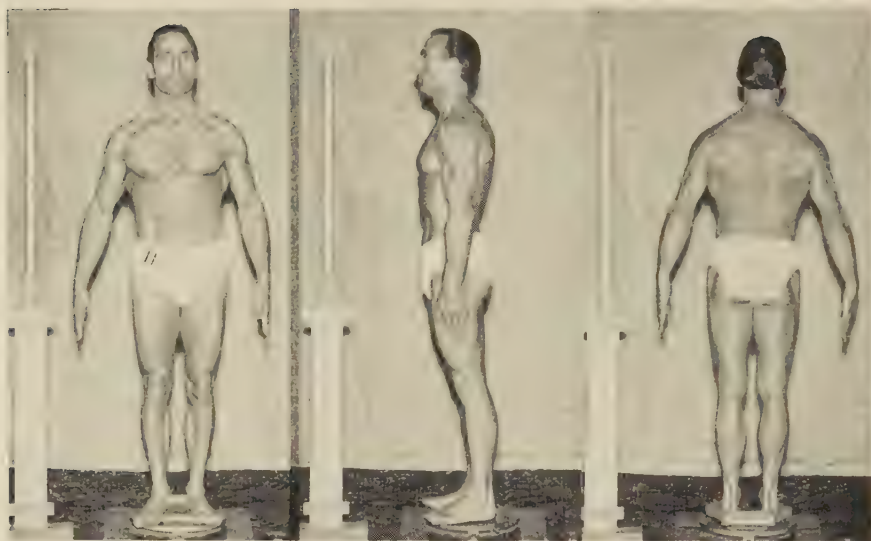


FIG. 2.—A “Mr. Universe” competitor, aged 42 years. Somatotype 172. R.E. Body Index $96 \cdot 8$.



FIG. 3.—Man aged 49. Somatotype 641. R.E. Body Index $96 \cdot 2$.

for women, for older adults of both sexes and for children. It remains to determine the degree of constancy of phenotype estimates derived in this way at successive ages. Will the phenotype, objectified in this way, prove constant enough to identify it with Sheldon's somatotype, remembering that the somatotype remains constant through life in so far as it succeeds, according to definition, in being a good estimate of morphogenotype? This important question remains

as yet unanswered. The answer will not, however, affect the descriptive usefulness of age and sex standardized phenotypes for cross-sectional, as opposed to longitudinal, socio-medical surveys.

APPENDIX

Mean Somatotype and Body Index of Oxford Men Students and Mr. Universe Competitors

Oxford Men n=344				Mr. Universe Competitors n=23		
	Mean	Standard Deviation	Standard Error	Mean	Standard Deviation	Standard Error
Endomorphy ..	3·43	0·80	0·04	2·37	0·40	0·08
Mesomorphy ..	3·68	0·77	0·04	6·74	0·29	0·06
Ectomorphy ..	3·91	1·15	0·06	1·39	0·49	0·10
R.E. Index ..	107·03	6·81	0·37	95·5	3·08	0·64

The somatotype was assessed from photographs after preliminary estimates based on the standard deviation chart for young men (Parnell, 1954). The distribution of nine Mr. Universe competitors under 25 years of age did not differ significantly from older competitors.

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CONTROL OF PETIT MAL BY ACETAZOLAMIDE

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WE preface this short communication by emphasizing that it originates as a by-product from some work that is still in progress at the Burden Institute on the balance of electrolytes in various normal psychological and neurotic conditions. This research is chiefly being conducted by McLeod, Morrison and Fosbrooke, in the Biochemical Department, and Grey Walter in the electro-physiological laboratory.

In an attempt to co-ordinate its activities we were led to consider what help might be afforded by a study of the so-called epileptic crises. The results obtained in a certain type of discharge seemed to be of such therapeutic importance that we did not feel justified in withholding them till the biochemical studies had made further progress. We were the more constrained to do so in that in the past few months some valuable work published using the same type of carbonic anhydrase inhibitor, Acetazolamide, might give a rather false impression as to its value in epilepsy.

We presume that in the early years of the century most of us were intrigued by the apparent occasional success of the ketogenic diet, but it was not till the advent of electroencephalography that respiratory alkalization made the influence of pH on the occurrence of epileptic concomitants clear. Together with this there is a demonstrable inhibiting effect of carbon dioxide on some continuous epileptic discharges, and also we have the more ambiguous results of sympathetic conditioned cerebral vasoconstriction. There was, therefore, some excuse for anyone with therapeutic pre-occupations to turn attention to the possible effects of reduction of pH. The difficulty was to effect this by any practicable biochemical procedure. Any device permanently to raise the CO₂ percentage of the tidal air was obviously not to be thought of for practical purposes, though using as an experimental method a rather cumbersome high oxygen re-breathing apparatus one of us (F.L.G.) obtained some interesting results about fifteen years ago. Calcium chloride was not to be thought of, but as the same author has worked on gaseous metabolism the possibilities of ammonium chloride were naturally much in mind. Here, however, the homeostatic mechanism of the respiratory centre is sufficiently powerful to defeat any attempt to produce a permanent alteration of any significance and the drug itself in large doses is, of course, excessively unpleasant.

The implications of any fall of pH on the electrolyte balance make it obvious that the more direct method of specific electrolyte absorption by resins might at any rate clear up some of the problems of interpretation. Here again the bulk and general nauseousness of all the resins prepared in our laboratory made it evident that nothing could be hoped for from their use, even on an experimental scale. The possibility of producing an acid shift of the chemical milieu of the central nervous system by a carbonic anhydrase-inhibiting drug

was evidently not only likely to prove of value in the research work on electrolyte influence in psychological states, but gave an admirable method for some preliminary investigations on the chemical mechanism of epileptic discharges.

In attempting the assessment of the therapeutic value of any drug in this connection we should never forget that under the term epilepsy we are dealing with a congerie of disorders differing in causation, mechanism and symptomatology. As Kinnier Wilson put it—we must talk not of epilepsy but the epilepsies. It would seem that we are very much in the position of a clinician asked to deal with a hospital full of patients who could only be described in terms of the thermometer as suffering from fever. The analogy is less fantastic if we remember that just as such a clinician might be able to control temperature by cortisone, the neurologist can to a great extent control these crises of the nervous system by such drugs as paraldehyde, but in neither case could it be claimed that the causal conditions were dealt with.

The epileptic problem is further complicated by the irregularity of the crises, their susceptibility to changes in the internal somatic environment and in the external social and psychological environment. It is for these reasons that any attempt to assess the value of any drug with a small number of patients, certainly with less than a hundred, is liable to mislead.

The cases studied must be carefully grouped, not only as to their symptomatology and frequency of attacks, but as far as possible as to their obvious causation. Groups should contain a minimum of 50 patients. To record a diminution or increase of fits in a collection of fifty epileptics indiscriminately lumped together without any indication as to the clinical history and type of patient studied is only likely to lead to a very misleading assessment of the values of the drug employed. This is, of course, the history of so many conflicting assessments of the efficacy of drugs and treatment.

A further obvious requirement should be that any new drug under trial should be added to the treatment that the patient has been having for many months and psychological effect be avoided, both by placebo substitution and by in no circumstances removing the patients to new surroundings, such as admission into hospital for trial.

It is only in a very large clinic that these conditions can be fulfilled and at the Burden Institute, drawing our out-patients from wide areas in the West Country, and to a great extent from other parts, this has been possible. Cases from the Neuro-psychiatric Department at Musgrove Park Hospital, Taunton, have also been included.

It should be unnecessary to insist that all cases should be electroencephalographically controlled and seen at frequent intervals.

In order to effect the maximum inhibition of carbonic anhydrase Acetazolamide proved to be the most potent and least toxic available body. Its effects on the bodily pH have been much studied in connection with the treatment of glaucoma and its inhibiting power is several times greater than that of the much more toxic sulphonamides with which we attempted some inconclusive experiments on the control of epilepsy nearly ten years ago. A number of cases were chosen exhibiting the symptoms of centrencephalic epilepsy with the seizure pattern generally described as petit mal lapse (absence) and with a primary bilateral EEG synchrony exhibiting a wave and spike rhythm of about three per second. No cases were considered except those with a daily fit incidence of anything from two to a hundred a day.

In point of symptomatology it was decided to be severely eclectic. No cases of automatism, of akinetic or drop seizures, or emotional outbursts or myoclonic

petit mal were included. All cases had been refractory to all drug control and had been under the same drug treatment for at least two months, and under observation with all forms of drug treatment for at least six months.

The type of EEG in our cases has been described by Lennox in terms that could hardly be bettered. He says—"This usual rhythm of 10 to 20 discharges a second is a mosaic of the activity of many centres. In a petit mal many waves become one. The large waves represent not a suppression or an augmentation but rather a synchronization of various discharging cell clusters. There is integration of activity as regards timing, but disintegration as regards the delicate and complicated interplay of various discharging nuclei."

None of our cases had a traumatic history and none were mentally defective. Some two or three were rejected halfway through treatment on account of proved unreliability in drug taking or statements.

The patients were children and adults of both sexes with ages ranging from 6 to 35 years.

The total number of cases in the group was 78 and the minimum time of observation from the beginning of treatment was three months.

Of the 78 cases, all of which had proved refractory to every form of treatment, only two failed to respond to a 250 mg. daily tablet of acetazolamide added to their previous treatment. Thirty-four patients have completely lost their minor attacks during periods of observation of from three to ten months, and of the remaining cases the incidence of attacks dropped to one or two a month from an average of five or more a day. Of these cases the majority were free from attacks for the first month following acetazolamide administration and then a one or two attacks a month rhythm re-asserted itself. Increasing the acetazolamide dose to 500 or even 750 milligrams gave no significant diminution of attacks. But to come to any definite conclusion in cases with minors occurring only once or twice a month a longer period than ten months observation would obviously be desirable. In five of these cases with this low incidence reinforcement of the acetazolamide with ammonium chloride in .5 g. doses three times a day abolished the attacks altogether. It is noteworthy that abnormalities of the EEG response to stroboscope stimulation showed variation in the direction of improvement only a long time (often some weeks) after the clinical abolition of the petit mal attacks.

This is not the time to attempt any analysis of the pharmacology of acetazolamide. We are concerned in this short note to point out that as far as a particular type of epileptic syndrome goes the drug has an efficiency that would indicate its imperative utility in the treatment of petit mal. Other recent workers have indicated that it has undoubted anti-epileptic value but to record a reduction of fits in a small collection of unclassified cases tends to obscure the ultimate specific value of a drug and to delay its effective application. We suspect that neglect of this obvious source of error has led to some of the extreme divergences of opinion as to the relative value of other anti-epileptic remedies.

What may be the value of acetazolamide in other epileptic syndromes is at present being investigated but when dealing with types of fits occurring at regular intervals the therapeutic assessment is more difficult.

In many cases the definition of the type of fit syndrome is likely to be somewhat more difficult than in the case of petit mal. Some of our experiments on the protective power of acetazolamide against E.C.T. in rats has led us to doubt whether it is likely to be of any great value in treatment of cortically-conditioned epilepsy, but more extensive clinical observation and a rigorous analytic classification of the types of cases involved is necessary.

We have so far encountered no toxic side effects. In two cases among the cases of both petit mal and major epilepsy so far treated an ominous fall of the neutrophil count, but without other symptoms, was noted by our technicians, and on enquiry it turned out that both of these patients had shown a similar reaction to hydantoin medication. A number of patients noted tingling of hands and feet, but this symptom rarely persisted more than a few days. Twice somnolence was complained of, but again only for the first few days of treatment. No patients ever seemed to notice diuresis, and it is of interest that the diuresis only lasts for the first three days of treatment and hence dehydration cannot be an explanation of the effects of the drug.

A number of cases after complete absence of minor fits during the first couple of months of treatment would report one or two minors a month, but so far in our eleven months experience there has been no return to anything approaching the conditions obtaining before treatment was initiated.

The effect on petit mal incidence is noted within three to four hours of administration. The recurrence of petit mal may occur within twenty-four hours of cessation of administration of the drug.

The efficient dosage is hard to assess, as a rule a daily dose of 250 mg. is equally effective for children of less than five stone and adults of ten stone. We have not noted any increased efficacy when the drug is increased beyond 750 mg. per diem.

The relation of the increased potassium output, the greatly diminished alkali reserve and the alveolar air carbonic acid concentration form part, as we have already said, of the research being pursued at the Burden Neurological Institute on the general implications of variations of the electrolyte balance on the activity of the C.N.S. and the observations here detailed are to be regarded as a note on the purely clinical assessment of the effect of acetazolamide on a specific type of centrencephalic epilepsy.

Acetazolamide is prepared by Lederle, the American Cyanamid Company and known as Diamox. We have been greatly indebted to them for liberal supplies of the drug.

A CONTROLLED CLINICAL TRIAL OF METHYLPENTYNOL ("OBLIVON")

By

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THERE have been many new types of sedatives and hypnotics evolved in recent years. The term "tranquillizing drug" is a recent innovation which suggests a calming effect without a hypnotic effect. Methylpentynol ("Oblivon") is one such drug which has little hypnotic action, but is said specifically to relieve anxiety. This action was noted by the relief of apprehension observed in patients prior to dental treatment (Trotter, 1953). Methylpentynol is a higher alcohol which in high dosage has a hypnotic effect in animals (Margolin *et al.*, 1951). No toxic effects have been reported. It is made up in rather large, blue-green capsules containing 250 mg. of methylpentynol, and also as an elixir.

THE TRIAL

There has been no previous controlled trial of methylpentynol in psychiatric cases. In this trial patients with various psychiatric disorders were tried on the drug. Generally it was given to patients exhibiting tension, anxiety, fear and panic reactions, regardless of the psychiatric diagnosis. Some patients were in-patients in a psychiatric unit, others were attending psychiatric out-patients departments of general hospitals.

The usual method of administration of the capsules was to instruct the patient to take two capsules whenever he felt anxious, up to eight per day. He was also advised to take 2-4 capsules prophylactically before facing stress situations. This prophylactic action has previously been found of value in hospital practice (Boag, 1954). Patients were observed over periods ranging from six to eighteen months. Inert capsules of identical size, shape and colour were given to a similar control series of patients. A small number of patients was given a trial of both inert capsules and drug capsules. Thus a patient who responded well to the drug was tried on a course of the inert capsules. Similarly, a patient who did not respond to the inert capsules was given a course of the drug capsules.

RESULTS

The responses to the drug and inert capsules were classified into three groups:

A—Very effective.

B—Some benefit.

C—No benefit.

"Very effective" implies complete or marked relief of tension or anxiety.

"Some benefit" means a limited improvement in symptoms.

The results of the trial are summarized in Table I.

TABLE I				
	Group A	Group B	Group C	Total No. of
	Very Effective	Some Benefit	No Benefit	Patients
Drug capsules	31 (49)	13 (20)	20 (31)	64
Inert capsules	4 (17)	9 (39)	10 (44)	23
	(Percentages in brackets)			

It will be noted that about half the patients on the drug were considerably helped, and more than two-thirds (69 per cent.) obtained some relief of symptoms. A little under one-third had no response to the drug.

An example of the therapeutic action of the drug is shown in the chronically anxious patient who was able to go to a dance for the first time in years with confidence, and freedom from anxiety. Another patient was able to go on the stage, to face people, and to obtain a new job. He ascribed these "feats" to the drug. The drug appeared to be of particular value in *phobic* anxiety states, e.g. those patients who have difficulty in travelling, going out alone, or who fear closed spaces, or fear injuring others, etc. These phobias, associated with anxiety or sometimes panic states, were in some cases overcome or in others modified by the drug. Thus, two to four capsules before undertaking a journey was frequently helpful to several such patients. Another group who were helped were those patients who developed sudden acute panic attacks. Two to four capsules taken at the first sign of a panic reaction frequently aborted it.

One patient said his anxiety was relieved but he felt "rather numb". A patient who felt "on edge" in a crowd, had frequency of micturition and was afraid to travel any long distance, said he felt relaxed and could do things he could not normally do—"As though I'd had a stiff drink". Another patient said she felt calm, peaceful, less agitated and irritable. Blushing and shyness were considerably relieved in one patient, who was able to join in socially at a party—an unusual feat for her.

The claim that methylpentynol enables one to face the rigours of dental surgery with equanimity (Trotter, 1953) seemed to be amply confirmed in this series. One patient who constantly fainted in the dental chair was enabled to have teeth extracted with little anxiety. Another patient has been unable to have dental treatment without a general anaesthetic due to persistent severe retching while in the chair. For the first time in many years he was able to have several teeth extracted without a general anaesthetic, half an hour after having four methylpentynol capsules.

A patient with a phobic anxiety state was able to go swimming for the first time in 16 years, and to the cinema for the first time in a year. A detective policeman complained of tachycardia, dry mouth and dyspnoea when attending police court, or making an arrest. Two capsules taken half an hour before enabled him to make an arrest calmly and without symptoms. Tired, drunk, calm, peaceful, far-away feeling, confident, fearless, were some of the expressions used by patients in describing the effect of the drug.

THE INERT CAPSULES

The difficulties of assessment of drugs used in psychological medicine are shown by the therapeutic value of the inert capsules. Over half (56 per cent.) of the patients on inert capsules obtained some relief of symptoms, as shown in Table I. This figure is comparable with those on the drug capsules (56 per cent.: 69 per cent.), but it is noteworthy that in the two groups *very effectively* helped by the drug and inert capsules, the ratio of one to the other is nearly 3:1 in

favour of the drug capsules (Table I). So that while the effect of suggestion often produced some amelioration of symptoms, it was mainly with the *drug* capsules that symptoms were greatly relieved. A further point of interest is that all four of the cases who responded very well to inert capsules had a diagnosis of anxiety hysteria, with the hysterical component predominant.

Of the nine patients who were given both the drug and the inert capsules, four did better on the drug than on the inert capsules. None did better on the inert capsules than on the drug capsules. Five showed no significant difference in the effects of inert and drug capsules. These results are shown in Table II.

TABLE II		
Case	Drug Capsule	Inert Capsule
1	A	A
2	A	B
3	A	B
4	A	B
5	A	C
6	B	B
7	B	B
8	C	C
9	C	C

A=Very effective B=Some benefit C=No benefit

UNRESPONSIVE CASES

Amongst the cases *not* helped by methylpentynol was an agitated (psychotic) depression. The patient felt worse, became more restless and rather more depressed. As would be expected, he responded well to electroplexy. Another case apparently made worse was one of obsessive-compulsive-phobic state. The patient felt more fear of being "uncontrolled" when she took the drug. In contrast, sodium amytal was helpful in this case. (It has already been observed that phobic anxiety states generally seemed to respond best of all to the drug.). Several patients who were depressed complained of feeling either unrelieved or more depressed on the drug. One case of psychogenic impotence was not helped by the drug.

TOXIC EFFECTS

No serious toxic effects were observed in any of the 87 patients. The following side effects were observed: postural vertigo, mild nausea, anorexia, dyspepsia, belching "aromatic" wind, "heartburn", vomiting (one case), slight drowsiness, "feeling intoxicated". Eleven patients of the 87 complained of one or other of these side effects, and of these "repeating" or belching was the commonest symptom. There were no effects on the blood picture or urine. The drug, clearly, was practically non-toxic.

SUMMARY

A controlled trial of methylpentynol ("Oblivon") was conducted on 87 patients who were observed from six to 18 months.

The drug was given to relieve anxiety and two-thirds of the cases were improved or much improved.

The conditions best affected were anxiety states and phobic anxiety states. Phobias and acute panic states were often helped. The effect of suggestion was discussed, half the patients on inert capsules being symptomatically improved. But a very good response to the capsule was three times more frequent with the drug than with the inert capsule.

The drug did not often benefit cases where depression was prominent.

There were no toxic symptoms, and only minor side effects were observed.

Methylpentynol appears to be a useful rapidly acting drug for allaying apprehension and anxiety without any marked hypnotic effect. Its prophylactic action was valuable.

ACKNOWLEDGMENT

I am indebted to Messrs. British Schering Ltd., for kindly supplying me with the drug "Oblivon" and the inert capsules used in this trial.

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SERUM PROTEINS IN MONGOLISM

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In recent years electrophoretic methods for the analysis of the serum proteins have been extensively and fruitfully applied to studies of diseases of the nervous system (Volk, Saifer, Rabiner and Oreskes, 1955; Volk, Saifer, Rabiner and Hinterbuchner, 1956; Press, 1956).

Investigations of the serum proteins in mongolism by classical chemical methods failed to establish significant abnormalities (Benda, 1947). However, the possibility that a protein profile specific to mongolism might be demonstrable by the more sensitive electrophoretic techniques prompted us to reinvestigate the matter.

CLINICAL GROUPS

The investigation was carried out on 36 children with mongolism, each being paired with a mentally defective child as control. The pairs were matched for age, weight and diet, and drawn wherever possible from the same wards. Eighteen of the pairs fell in the age group 2-6 years and 18 in the age group 6-10 years. The mean I.Q. of the mongoloid children was 34, range 5-67, while that of the controls was 31, range 3-57.

The classification of the patients is shown in Table I. "Unclassified" here means that the mental defect could not be attributed to any specific cause. Five of the patients in this sub-group showed psychotic features. As far as could be ascertained all patients were free from infection.

TABLE I
Classification of Cases

Mongolism ..	..	..	..	..	..	..	36
Cerebral palsy ..	..	..	..	..	..	..	6
Birth injury ..	..	..	..	..	..	..	4
Tuberculous meningitis ..	..	..	..	..	..	..	1
Icterus gravis neonatorum ..	..	..	..	..	..	..	2
Microcephaly ..	..	..	..	..	..	..	4
Epilepsy ..	..	..	..	..	..	..	2
Unclassified ..	..	..	..	..	..	..	17

METHODS

Collection of Samples

Capillary blood was used throughout. About 0.2 ml. of blood was obtained from a finger prick and allowed to clot in a $\frac{3}{8}$ in. tube. Half an hour after clotting had taken place the serum was separated by centrifuging for 10 minutes at 2,500 rev./min.

Electrophoresis on Paper

The experiments were carried out within half an hour of the separation of the serum by the horizontal open strip technique (Grassmann and Hannig, 1952). 0.01 ml. of serum was applied by a $1\frac{1}{4}$ inch microscope slide to strips of Whatman No. 1 paper and run for 5 hours at 2° C. in barbiturate buffer pH 8.6, ionic strength 0.05, at a current of 0.3 mA/cm. and a potential of approximately 17 V/cm. length. To minimize evaporation the tank was fitted with a rubber gasket.

At the end of the run the proteins were denatured by heating in an oven for 30 minutes at 110–120° C. and stained in a 0.5 per cent. solution of light green S.F. (Hopkin and Williams Ltd.) in water containing 25 per cent. ethanol and 5 per cent. acetic acid (Dangerfield and Smith, 1955). After clearing of the background with 2 per cent. acetic acid and drying, the strips were made translucent by immersion in a mixture of 20 vol. liquid paraffin, 12 xylene, 10 bromonaphthalene and 1 per cent. Tween 80 special grade (Honeywill and Stein Ltd.), (Rees and Laurence, 1955). After surface drying with filter paper the strips were scanned with an EEL scanner (Evans Electro selenium Ltd.) using a Chance OY4 yellow filter.

From experiments with Armour bovine serum albumin and γ globulin it was found that in our experimental conditions albumin absorbed 1.3 times as much dye as globulin. This was corrected for in the computation of the concentration of the serum protein fractions. Deviations from Beer's law were corrected for by the method of Latner (1955). No correction was made for the albumin tail. Samples from mongols and their matched controls were without exception taken together through the entire estimation. In this way conditions for electrophoresis, protein denaturation, staining and scanning were identical for each pair.

Chemical Methods

Total proteins were estimated by precipitation from a 1 : 500 dilution of serum in saline with an equal volume of 10 per cent. trichloroacetic acid, followed by digestion with a 10 per cent. solution of selenium in concentrated sulphuric acid for half an hour, and Nesslerization.

Zinc sulphate and thymol turbidity tests were carried out according to Kunkel (1947) and Maclagan (1944) and the turbidities read against Mestrezet standards (Baird and Tatlock Ltd.).

RESULTS

The mean values for the protein fractions in the sera of the mongols and controls are shown in Table II.

While the total proteins are almost identical in both groups, the albumin is significantly lowered and the γ globulin raised in patients with mongolism. The lowering of the serum albumin is entirely due to the highly significant difference in the 6–10 year group; there is no significant difference in the age group 2–6 years.

When the two age-groups are compared (Table III) it is found that the α_1 , α_2 and β globulin, but not the γ globulin, are lower in the 6–10 year age group, both in controls and in patients with mongolism. In the former, but not in the latter, the serum albumin is higher in the 6–10 year group.

Experiments with the zinc sulphate and thymol turbidity tests (Table IV) are fully consistent with the electrophoretic measurements, higher turbidity readings, although still within the normal range, being found in patients with

TABLE II

Serum Protein Fractions Determined by Paper Electrophoresis and Total Proteins in the Sera of Mongoloid Children and of Other Mentally Retarded Children as Controls. Significant Differences ($P < 0.05$) are Shown in Heavy Type

Protein Fraction	Mongols			Controls			Significance of Differences		
	N	Mean Per-centage of T.P.	S.E.	N	Mean Per-centage of T.P.	S.E.	Δ	t	P
bumin:									
Age 2-10 years	36	48.3	0.77	36	53.4	0.84	5.1	>3.460	<0.001
Age 2-6 years	18	46.9	1.85	18	50.3	1.15	3.4	<1.684	>0.1
Age 6-10 years	18	49.6	1.04	18	56.4	0.92	6.8	>3.646	<0.001
α_1 globulin:									
Age 2-10 years	36	6.1	0.34	36	6.2	0.23	0.1	<1.671	>0.1
Age 2-6 years	18	6.7	0.29	18	6.7	0.27	0.0	<1.684	>0.1
Age 6-10 years	18	5.5	0.35	18	5.6	0.34	0.1	<1.684	>0.1
α_2 globulin:									
Age 2-10 years	36	10.9	0.29	36	11.1	0.31	0.2	<1.671	>0.1
Age 2-6 years	18	11.7	0.44	18	11.9	0.34	0.2	<1.684	>0.1
Age 6-10 years	18	10.1	0.22	18	10.3	0.43	0.2	<1.684	>0.1
β globulin:									
Age 2-10 years	36	13.1	0.28	36	12.6	0.27	0.5	<1.671	>0.1
Age 2-6 years	18	13.6	0.39	18	13.5	0.30	0.1	<1.684	>0.1
Age 6-10 years	18	12.6	0.23	18	11.7	0.33	0.9	>2.042	<0.05
γ globulin:									
Age 2-10 years	36	21.7	0.75	36	16.7	0.53	5.0	>3.460	<0.001
Age 2-6 years	18	21.1	1.28	18	17.5	0.84	3.6	>3.646	<0.001
Age 6-10 years	18	22.3	0.85	18	16.0	0.50	6.3	>3.646	<0.001
Total Proteins (g./100 ml.):	N	Protein level	S.E.	N	Protein level	S.E.	Δ	t	P
Age 2-10 years	35	6.58	0.09	36	6.59	0.07	0.01	<1.671	>0.1
Age 2-6 years	18	6.58	0.12	18	6.65	0.12	0.07	<1.684	>0.1
Age 6-10 years	17	6.58	0.14	18	6.53	0.08	0.05	<1.684	>0.1

TABLE III

Comparison of Serum Protein Fractions Determined by Electrophoresis, and Total Proteins in Two Age Groups of Mongoloid Children and Other Mentally Retarded Children as Controls. N=18 except where Otherwise Stated. Significant Differences ($P < 0.05$) are Shown in Heavy Type

Protein Fractions	Age 2-6 Years		Age 6-10 Years		Significance of Differences		
	Mean Percentage of T.P.	S.E.	Mean Percentage of T.P.	S.E.	Δ	t	P
Albumin:							
Mongols	46.9	1.85	49.6	1.04	2.7	<1.684	>0.1
Controls	50.3	1.15	56.4	0.92	6.1	>3.646	<0.001
α_1 globulin:							
Mongols	6.7	0.29	5.5	0.35	-1.2	2.457	<0.02
Controls	6.7	0.27	5.6	0.34	-1.1	2.457	<0.02
α_2 globulin:							
Mongols	11.7	0.44	10.1	0.22	-1.6	>2.750	<0.01
Controls	11.9	0.34	10.3	0.43	-1.6	>2.750	<0.01
β globulin:							
Mongols	13.6	0.39	12.6	0.23	-1.0	>2.042	<0.05
Controls	13.5	0.30	11.7	0.33	-1.8	>3.646	<0.001
γ globulin:							
Mongols	21.1	1.28	22.3	0.85	+1.2	<1.684	>0.1
Controls	17.5	0.84	16.0	0.50	-1.5	<1.684	>0.1
Total Proteins (g./100 ml.):	Protein level	S.E.	Protein level	S.E.	Δ	t	P
Mongols	6.58	0.12	6.58*	0.14	—	<1.697	>0.1
Controls	6.65	0.12	6.53	0.08	-0.12	<1.697	>0.1

TABLE IV

Zinc Sulphate and Thymol Turbidity Reactions in the Sera of Mongoloid Children and of Other Mentally Retarded Children as Controls

Turbidity Reaction	Mongols			Controls			Significance of Differences		
	N	Mean (Mestrezet Units)	S.E.	N	Mean (Mestrezet Units)	S.E.	Δ	t	P
Zinc sulphate	20	2.50	0.18	20	1.40	0.09	1.10	>3.551	<0.001
Thymol	20	1.45	0.28	20	0.83	0.11	0.62	>3.551	<0.001

* N=17.

mongolism. This one would expect as it is particularly the level of the γ globulin that is reflected in these precipitation reactions.

DISCUSSION

Owing to the ease with which small differences, particularly in staining and scanning techniques, can affect the quantitative analysis of serum proteins by paper electrophoresis (Martin and Franglen, 1954; Schulz and Holdcroft, 1956), great caution must be exercised in comparing results obtained in different laboratories. Bearing this in mind the data for our controls in the 6-10 year age group are in satisfactory agreement with data for normal children in the literature (Caspari, Negri and Sticca, 1953; Plückthun, 1953).

The changes with age in the protein distribution are of some interest, particularly in the control group. In normal children the serum albumin and the α and β globulin reach their adult levels at an age of about 2 years (Caspari, Negri and Sticca, 1953; Orlandini, Sass-Kortsak and Ebbs, 1955; Plückthun, 1953). The significant differences in the levels of these proteins between the 2-6 and 6-10 year olds (Table III) show that the protein pattern has not yet become stabilized in the 2-6 year group. In mentally defective children adult levels are thus reached at a much later age than in normal children. This is not surprising as the physiological age of many of the children was evidently well below their chronological age. The mongoloid children are even more retarded with respect to their serum protein pattern, particularly the rise in the serum-albumin level with age, than the controls. This at least partly accounts for the fact that differences between mongols and controls in the albumin and β globulin are significant in the 6-10 year age group but not significant in the 2-6 year group. The γ globulin, in contrast, reaches stable levels in both controls and mongols if anything earlier than in normal children.

The protein profile in mongolism with its raised γ globulin and lowered albumin is unlike any observed so far in patients with other lesions of the central nervous system (Volk, Saifer, Rabiner and Hinterbuchner, 1956; Press, 1956). It resembles that found as an aftermath of an infection, in chronic infections, in conditions of suboptimal liver function and in some cases of leukaemia (Flynn, 1954; Martin and Franglen, 1954; Vera and Roche, 1956). It has also been suggested (Lewis and Page, 1956; Page, Lewis and Gilbert, 1956) that genetic factors may affect the relative proportion of albumin and γ globulin.

There was no detectable difference in the general state of health of our patients with mongolism and controls, so that it is improbable that a greater prevalence of infections in the former at the time of test could account for the observed differences. The raised level of the γ globulin is of particular interest. According to modern views γ globulin is complex biologically and physico-chemically. It is synthesized in several types of cell in the bone marrow, spleen and lymph nodes (Askonas, Humphrey and Porter, 1956). A raised level is associated with hyperactivity of the reticulo-endothelial and/or haematopoietic systems. The high γ globulin would therefore fit in well with the "shift to the left" in the polymorphonuclear leucocytes (Turpin and Bernyer, 1947; Shapiro, 1949; Mittwoch, 1956) and with the increased tendency to leukaemia postulated by Krivit and Good (1956) and Merrit and Harris (1956). According to these authors the severe injury to the foetus in the first 6-7 weeks which results in mongolism may affect the bone marrow and haematopoietic system as well as any of the other anatomic structures undergoing rapid differentiation at that time, e.g. brain, heart and skeleton. This might well result in a "shift to the left", and if the injury is exceptionally severe, in leukaemia. Speculation on

how far the observed protein pattern is caused by suboptimal liver function and how far indirectly by injury to the haematopoietic system appears profitless at this stage. It is hoped that experimental work now in progress on other aspects of metabolism in mongoloid children will throw light on this matter.

SUMMARY

1. The serum albumin was decreased and the serum γ globulin increased in a group of 36 mongoloid children compared to a matched group of controls. There was no significant difference in the total proteins, α_1 , α_2 , and β globulins.
2. Higher turbidity readings, though still within the normal range were also found for the children with mongolism in the zinc sulphate and thymol turbidity tests.
3. When a comparison was made between the age groups 2-6 years and 6-10 years significant differences were observed in all the globulins except the γ globulin both in the mongoloid children and in the controls. In the latter, but not in the former group, there was also a significant difference in the serum albumin.
4. The possible significance of these findings is briefly discussed.

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INITIAL PSYCHIATRIC ILLNESS IN INVOLUTIONAL WOMEN

II. PSYCHOLOGICAL ASPECTS

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INTRODUCTION

THE following paper describes the psychological part of a psychiatric and EEG study of psychiatric illness occurring for the first time in women of the involutional age range. The origins of the investigation and the clinical results are fully discussed elsewhere (Tait *et al.*, 1957). They include such matters as the psychiatric observation that classical forms of melancholia seem to have become relatively infrequent. One possible explanation for this may be thought to be in the earlier recognition, hospitalization and treatment of psychiatric illness. It was decided that an appropriate study would pay attention only to women who are admitted to a mental hospital for the first time. In practice this has included all such women admitted to amenity beds in the hospital over a given period of time. This modified selection of admissions was made in order to ensure as wide a range of illness as possible, giving a higher proportion of neurotic illness without excluding psychotic material. The age range studied was that usually considered to encompass the normal range of the biological involution, 40–55. Such a study thus avoids the bias (apart from age) of any preconceived mode of selection. By including only first admissions, some assurance was given that up till then, the patients had been reasonably free from psychiatric illness.

METHOD

To avoid preconceptions affecting the results, techniques as open and unstructured as possible were required. Those used were a Standard Psychological Interview, the Thematic Apperception Test, and the Rorschach Test. Fifty-four female patients agreeing with the required criteria were interviewed, and of these, fifty-two also completed Thematic Apperception and Rorschach Test performances. Each patient was seen soon after admission to hospital, and prior to treatment. An urgent need for treatment accounts for the loss of two T.A.T. and Rorschach performances. The full T.A.T. of 20 cards was used, as the absence of adequate norms prevents a satisfactory reduction of the test material, although desirable. Murray's (1938) original method of presentation of the T.A.T. was followed. With this method, no reformulation of the test's requirements are made to the subject, except after the first card. Similarly, no routine pursuit questions are asked, as in some subsequent work (e.g. Rapaport, 1946). The complete psychological investigation of an individual usually involved two or three sessions (but sometimes more) each of about an hour's duration, the exact number and duration naturally varying with the individual.

With each technique, assessment was made by a comparative matching of the original, individual responses that had been given. This consisted of comparing each individual's response to a given Interview topic (or T.A.T. or

Rorschach card) with every other individual's response to that topic, and grouping them according to their similarities and differences. This type of method has previously been usefully demonstrated with the Standard Interview (McAdam and Orme, 1954), the T.A.T. (Foulds, 1953), the Rorschach test (Orme, 1956), and the procedure here is derived from this previous work.

With the sample studied here, all responses having an incidence of at least 25 per cent. N are listed in Table I under I. When this is done, a score can be

TABLE I

(1) INTERVIEW TOPICS

INTERVIEW TOPICS					I	A : B		C : D	
					N= 54	26	26	25	27
<i>Appearance and Behaviour:</i>									
Average height	..	..	..	..	.. 32	<u>21</u>	<u>10</u>	<u>19</u>	<u>12</u>
Short in height	..	..	..	..	.. —	<u>4</u>	<u>9</u>	<u>3</u>	<u>10</u>
Average build	..	..	..	..	.. 28	<u>12</u>	<u>15</u>	<u>14</u>	<u>13</u>
Thickset build	..	..	..	..	.. —	<u>9</u>	<u>1</u>	<u>3</u>	<u>7</u>
She seemed apprehensive, nervous, anxious	..				.. 20	<u>10</u>	<u>10</u>	<u>8</u>	<u>12</u>
She cried and was distressed	..	..			.. 16	<u>11</u>	<u>4</u>	<u>7</u>	<u>8</u>
She was on the whole calm	..	..			.. 15	<u>7</u>	<u>7</u>	<u>9</u>	<u>5</u>
She was co-operative	..	..	..		.. 28	<u>18</u>	<u>9</u>	<u>11</u>	<u>16</u>
Her co-operation was limited	..	..	..		.. 24	<u>8</u>	<u>16</u>	<u>12</u>	<u>12</u>
Her speech was coherent	..	..	..		.. 51	<u>26</u>	<u>23</u>	<u>22</u>	<u>27</u>
Her speech was fluent	..	..	..		.. 46	<u>22</u>	<u>22</u>	<u>20</u>	<u>24</u>
There was a reasonable amount of conversation					28	<u>18</u>	<u>9</u>	<u>14</u>	<u>13</u>
Her conversation was limited	..	..			.. 18	<u>2</u>	<u>15</u>	<u>9</u>	<u>8</u>
<i>She said:</i>									
<i>Her interests were:</i>									
In her house and home	..	..	..		.. 40	<u>18</u>	<u>21</u>	<u>19</u>	<u>20</u>
In sporting activities	..	..	..		.. 13	<u>7</u>	<u>6</u>	<u>9</u>	<u>4</u>
In reading, listening to the radio	..	..	..		.. —	—	—	<u>5</u>	<u>7</u>
In the cinema and theatre	..	..	..		.. —	—	—	<u>7</u>	<u>4</u>
In clubs and social affairs	..	..	..		.. 14	<u>9</u>	<u>5</u>	<u>8</u>	<u>6</u>
Curtailed because of her home, or were too few					14	<u>8</u>	<u>6</u>	<u>8</u>	<u>6</u>
Felt to have been dropped recently	..	..			.. 14	<u>7</u>	<u>7</u>	<u>5</u>	<u>9</u>
<i>Her relationships with others:</i>									
Were sometimes difficult	..	..	..		.. 38	<u>18</u>	<u>19</u>	<u>19</u>	<u>18</u>
Were very good	..	..	..		.. 18	<u>10</u>	<u>8</u>	<u>10</u>	<u>8</u>
She preferred being with others	..	..	..		.. 30	<u>15</u>	<u>14</u>	<u>15</u>	<u>14</u>
She preferred being by herself	..	..	..		.. 16	<u>11</u>	<u>4</u>	<u>8</u>	<u>7</u>
<i>Her past life:</i>									
Had been reasonable	..	..	..		.. 16	<u>9</u>	<u>7</u>	<u>8</u>	<u>8</u>
Had been difficult	..	..	..		.. 19	<u>10</u>	<u>8</u>	<u>9</u>	<u>9</u>
She had regrets about it	..	..	..		.. 15	<u>7</u>	<u>7</u>	<u>8</u>	<u>6</u>
<i>For the future:</i>									
She just wanted to return home	..	..	..		.. 30	<u>15</u>	<u>15</u>	<u>14</u>	<u>16</u>
She was apprehensive	..	..	..		.. —	—	—	<u>3</u>	<u>8</u>

TABLE I (continued)

TABLE 1 (continued)						I	A : B		C : D		
<i>Her illness consisted of:</i>											
	Being apprehensive, anxious, nervous	..	..	..	..	27	15	11	11	15	
	Feeling depressed	..	..	..	..	26	16	9	10	15	
	Feeling weak and run down, tired	..	..	..	..	17	11	6	7	10	
	A physical complaint	..	..	..	..	34	19	14	18	15	
	Being self-blamatory and expressing guilt	..	..	..	..	—	—	—	—	7	
<i>Her illness had been caused:</i>											
	By a physical complaint	..	..	..	..	14	10	4	7	7	
	By marital (or equivalent) disharmony	..	..	..	..	15	6	7	5	8	
<i>Her illness had lasted:</i>											
	A few months only	..	..	..	..	29	18	10	12	16	
	A number of years	..	..	..	..	14	7	7	9	5	
	Her concentration was impaired	..	..	..	..	41	21	19	15	25	
	Her memory was impaired	..	..	..	..	27	16	10	13	13	
	Her will power was strong	..	..	..	..	32	16	16	14	18	
	Her will power was weak	..	..	..	..	14	6	7	6	7	
(2) T.A.T. THEMES						N=	52	26	26	25	27
1	Boy is thoughtful—concentrating	..	..	..	..	24	12	12	9	15	
	Violin is broken or misperceived	..	..	..	..	13	4	9	5	8	
	Boy is unhappy, lonely, depressed	..	..	..	..	—	8	2	1	9	
2	A man farming	..	..	..	..	50	25	25	25	25	
	Married to the woman	..	..	..	..	27	17	10	14	13	
	He is the girl's father	..	..	..	..	24	15	9	10	14	
	She is the girl's mother	..	..	..	..	27	16	11	14	13	
	A schoolgirl or student	..	..	..	..	26	10	16	9	17	
	Who is not interested in practical things	..	..	..	..	18	14	4	10	8	
3 GF	The woman is in grief and despair	..	..	..	..	50	25	25	24	26	
	She is trying to open the door	..	..	..	..	13	7	6	8	5	
	Alternative themes given	..	..	..	..	—	7	1	—	—	
4	The woman is pleading with the man	..	..	..	..	35	16	19	14	21	
	The man wants to leave her	..	..	..	..	17	9	8	6	11	
5	A domestic scene	..	..	..	..	34	18	16	19	15	
	The woman is apprehensive	..	..	..	..	—	8	3	4	7	
	The books noted	..	..	..	..	—	—	—	7	5	
	The table noted	..	..	..	..	—	—	—	7	3	
6 GF	The woman has been startled	..	..	..	..	38	22	16	18	20	
	The man is her husband	..	..	..	..	13	7	6	6	7	
	A domestic scene	..	..	..	..	—	—	—	2	7	
7 GF	The woman is the girl's mother	..	..	..	..	41	20	21	21	20	
	She is reading to the child	..	..	..	..	30	16	14	17	13	
	The child is inattentive	..	..	..	..	22	14	8	12	10	
	The child is definitely unhappy	..	..	..	..	13	6	7	6	7	
	The doll is noted	..	..	..	..	31	13	18	12	19	

TABLE I (continued)

			I	A : B	C : D
8 GF	The woman is unspecifically reflective		25	<u>11</u> <u>14</u>	<u>11</u> <u>14</u>
	She is reflecting on the past		—	<u>8</u> <u>1</u>	— —
	She is reflecting on the future		—	— —	<u>7</u> <u>4</u>
	She is sad		—	— —	<u>3</u> <u>7</u>
	It might be a pose for/or a picture		16	<u>10</u> <u>6</u>	<u>7</u> <u>9</u>
9 GF	The left-hand woman is running away		23	<u>15</u> <u>8</u>	<u>12</u> <u>11</u>
	She is frightened		13	<u>10</u> <u>3</u>	<u>8</u> <u>5</u>
	The right-hand woman is watching, hiding		20	<u>12</u> <u>8</u>	<u>11</u> <u>9</u>
	She is definitely spying		—	<u>8</u> <u>4</u>	<u>7</u> <u>5</u>
	It is at the seashore		19	<u>12</u> <u>7</u>	<u>9</u> <u>10</u>
	The two women are peacefully together		15	<u>6</u> <u>9</u>	<u>5</u> <u>10</u>
	They are having a picnic, bathing		15	<u>5</u> <u>10</u>	<u>7</u> <u>8</u>
10	A couple embracing		34	<u>20</u> <u>14</u>	<u>17</u> <u>17</u>
	They are middle aged or elderly		17	<u>12</u> <u>5</u>	<u>10</u> <u>7</u>
	Left-hand person comforts the right-hand		—	<u>9</u> <u>1</u>	— —
	Both may be men		13	<u>3</u> <u>10</u>	<u>4</u> <u>9</u>
11	Trees noted		28	<u>15</u> <u>13</u>	<u>14</u> <u>14</u>
	Stones, rocks		30	<u>15</u> <u>15</u>	<u>13</u> <u>17</u>
	A bridge		18	<u>14</u> <u>4</u>	<u>12</u> <u>6</u>
	A snake		31	<u>19</u> <u>12</u>	<u>18</u> <u>13</u>
	Animals on bridge		16	<u>11</u> <u>5</u>	<u>9</u> <u>7</u>
	Rushing water		15	<u>9</u> <u>6</u>	<u>4</u> <u>11</u>
	A pathway		13	<u>7</u> <u>6</u>	<u>7</u> <u>6</u>
12 F	A middle aged or elderly person		23	<u>11</u> <u>12</u>	<u>11</u> <u>12</u>
	Who is evil, horrible looking		23	<u>13</u> <u>10</u>	<u>10</u> <u>13</u>
	She is a nun (or monk)		14	<u>6</u> <u>8</u>	<u>7</u> <u>7</u>
	The young woman is "good"		—	— —	<u>2</u> <u>7</u>
	The young woman is "bad"		—	— —	<u>7</u> <u>3</u>
13 MF	The man has murdered the woman		24	<u>11</u> <u>13</u>	<u>11</u> <u>13</u>
	He is crying, grief-stricken		16	<u>8</u> <u>8</u>	<u>8</u> <u>8</u>
	He is remorseful		14	<u>9</u> <u>5</u>	<u>8</u> <u>6</u>
	Intercourse has taken place		13	<u>8</u> <u>5</u>	<u>6</u> <u>7</u>
14	The contrasting light and shade noted		30	<u>17</u> <u>13</u>	<u>15</u> <u>15</u>
	The person is a man		26	<u>15</u> <u>11</u>	<u>14</u> <u>12</u>
	The person is not defined		15	<u>7</u> <u>8</u>	<u>5</u> <u>10</u>
	He is looking through a window		26	<u>15</u> <u>11</u>	<u>12</u> <u>14</u>
	He is getting out of a window		15	<u>9</u> <u>6</u>	<u>8</u> <u>7</u>
15	A graveyard		43	<u>24</u> <u>19</u>	<u>20</u> <u>23</u>
	A figure in grief mourning		15	<u>12</u> <u>3</u>	<u>10</u> <u>5</u>
	A ghost, spectre		13	<u>5</u> <u>8</u>	<u>4</u> <u>9</u>
	A man (unelaborated)		—	<u>7</u> <u>4</u>	— —
	It is horrible		—	— —	<u>2</u> <u>9</u>
16	A country scene		27	<u>16</u> <u>11</u>	<u>15</u> <u>12</u>
	Related to the past		19	<u>8</u> <u>11</u>	<u>9</u> <u>10</u>
	Containing children		16	<u>8</u> <u>8</u>	<u>8</u> <u>8</u>
	"Home"		—	— <u>9</u>	— —

TABLE I (continued)

						I	A : B	C : D
17 GF	A girl or woman	..	..	..	..	37	21 16	21 16
	Looking out or over a bridge	..	..	..	..	30	17 13	16 14
	Men carrying things	..	..	..	..	22	14 8	13 9
	They are pirates or smugglers	..	..	..	..	13	8 5	7 6
	The sun	..	..	..	..	19	11 8	9 10
	A house	..	..	..	..	13	9 4	7 6
	A boat	..	..	..	..	17	11 6	10 7
	A bridge	..	..	..	..	18	11 7	9 9
	Water	..	..	..	..	22	12 10	8 14
18 GF	A mother and child	..	..	..	..	22	15 7	15 7
	Right hand person is killing the left	..	..	..	..	—	— —	4 7
	The left hand person is ill	..	..	..	..	14	9 5	11 3
	The right hand person is sorrowful	..	..	..	..	16	11 5	9 7
	One person is a man	..	..	..	..	—	2 8	7 3
	The stairs are noted	..	..	..	..	25	9 16	13 12
19	A house in snow	..	..	..	..	29	17 12	15 14
20	A figure	..	..	..	..	42	21 21	22 20
	Tree or trees	..	..	..	..	23	15 8	11 12
	A fence	..	..	..	..	18	11 7	9 9
	Lamp or lights	..	..	..	..	13	8 5	7 6
	Darkness	..	..	..	..	14	8 6	6 8
(3)	RORSCHACH RESPONSES					N= 52	26 26	25 27
I	A winged whole (W PFM A)	..	..	..	..	42	22 20	17 25
	Whole of anatomical content (W F At)	..	..	..	..	13	8 5	8 5
	Human response to centre detail (D F H)	..	..	..	..	—	7 1	— —
II	Whole two animals (W PFM A)	..	..	..	..	37	21 16	19 18
	All responses to beak detail (D F arch.)	..	..	..	..	—	7 3	— —
	Perplexity over red colouring	..	..	..	..	—	9 1	7 3
III	Whole two humans (W PM H)	..	..	..	..	35	23 12	18 17
	Bow, butterfly response (D ^p F A, obj.)	..	..	..	..	19	9 9	8 10
	Whole two animals (W FM A)	..	..	..	..	—	— —	4 7
	Animal response to top red (D F A)	..	..	..	..	—	7 2	— —
IV	Animal skin (W Fc A)	..	..	..	..	17	7 10	8 9
	Other animal responses (W A)	..	..	..	..	22	14 8	10 12
	Unclassified details	..	..	..	..	15	10 5	— —
	Strong expressions of surprised distaste	..	..	..	..	—	— —	8 3
V	Winged whole (W PFM A)	..	..	..	..	38	19 19	17 21
VI	Animal skin (W PFc A)	..	..	..	..	22	16 6	12 10
	All other wholes	..	..	..	..	20	11 9	13 7
	Object response to top detail (D F obj.)	..	..	..	..	14	10 4	9 5
	Unclassified detail	..	..	..	..	13	7 6	— —

TABLE I (*continued*)

			I	A : B	C : D
VII	Animal response to top details (D F A) ..	..	17	<u>11</u> <u>6</u>	<u>11</u> <u>6</u>
	All responses to small bottom centre detail (d) ..	..	14	<u>10</u> 4	<u>13</u> 1
	Whole Chiaroscuro (W K) ..	..	13	9 4	7 6
	Whole crude texture (W cF) ..	..	15	3 <u>12</u>	<u>5</u> <u>10</u>
VIII	Animals response (D ^P FM A) ..	..	45	25 <u>20</u>	22 <u>23</u>
	Form colour response, bottom detail (D FC pl.)		21	<u>12</u> <u>9</u>	<u>12</u> <u>9</u>
	Tree response, top detail (D F pl.) ..	..	15	<u>10</u> <u>5</u>	<u>10</u> <u>5</u>
	All response to "flags" area (D) ..	..	—	8 2	— —
	All whole responses (W CF) ..	..	16	5 <u>11</u>	8 8
IX	Responses to top details (D) ..	..	24	<u>13</u> <u>11</u>	<u>15</u> <u>9</u>
	Responses to bottom detail (D) ..	..	14	<u>9</u> <u>5</u>	<u>9</u> <u>5</u>
	Responses to green detail (D) ..	..	—	7 3	— —
	Responses to centre detail (D) ..	..	—	7 1	— —
	All whole responses (W CF) ..	..	16	<u>9</u> <u>7</u>	<u>9</u> <u>7</u>
X	"Spider" responses (D ^P FA) ..	..	21	12 <u>9</u>	<u>9</u> <u>12</u>
	"Caterpillar" responses (D ^P F A) ..	..	16	<u>8</u> <u>8</u>	<u>7</u> <u>9</u>
	"Rabbit's head" responses (D ^P F Ad) ..	..	—	8 2	— —
	Outer yellow detail (D FC pl.) ..	..	14	8 6	6 8
	Top grey animal responses (D F A) ..	..	13	<u>8</u> 5	8 <u>5</u>
	Outer brown detail responses (D) ..	..	—	<u>7</u> 5	3 <u>9</u>
	Pink detail responses other than human (D) ..	..	—	<u>7</u> 2	— —
	Unclassified detail ..	..	17	<u>14</u> 3	<u>11</u> 6
	All whole responses (W CF) ..	..	22	<u>13</u> <u>8</u>	<u>14</u> <u>7</u>

computed for each individual by adding together the frequencies as tabulated of all responses which she actually gave. By this means, each individual can be given an Interview score, a T.A.T. score, and a Rorschach test score. The highest scoring individuals can be considered the most typical or conforming individuals, as they give more of the responses common in the sample. Each individual's score with any technique was then expressed as a percentage of the highest scoring individual in each case. This evens out the range and size of scores between the three techniques, and avoids giving an undue weighting to any one technique. Adding each individual's three scores together (for the 52 completing all three techniques), a total score for each is obtained which can be considered as expressing the degree to which each individual conforms to the sample as a whole. Having done this, the top scoring half can be separated from the bottom scoring half. Each half can now be considered as constituting the most conforming and the least conforming groups respectively, of the sample. Here these groups will be referred to as Group A and Group B respectively.

A new comparative assessment of the responses of Groups A and B was now carried out—responses being considered common in either group if they had at least a 25 per cent. frequency in that group. Responses were further considered typical of one group in contrast to the other if, in addition, their frequency was at least double its occurrence in the other group. These responses are detailed in Table I under A and B, being underlined if common, and given in heavy type if typical. Each individual can now be given a score on a Group A–Group B continuum. This is calculated by adding together all A minus B

listed frequencies for the responses she actually gave. For example, if an individual was of average height and of average build, her A-B score for these two responses would be $(21-10)+(12-15)=8$, and so on for all her responses to each of the three techniques. When each individual's score has thus been computed, an estimation of the success of this method of scoring in distinguishing the groups can be made. The top scoring half should be predominantly Group A, and the bottom scoring half should be predominantly Group B. The Interview, in fact, successfully distinguishes 81 per cent., the T.A.T. 88 per cent., and the Rorschach 81 per cent. The range and size of scores for each of the three techniques can be made roughly equivalent, to avoid giving undue weighting to any, and then added together. With this summated score a 96 per cent. level of success in differentiating Groups A and B is obtained. The difference in means of scores for the two groups is naturally highly significant.

The content of the responses contributing most to these numerical differences will be abstracted and discussed later in the paper. The rationale of this method is briefly illustrated by comparison with intelligence testing procedures. The final summated intelligence test score of an individual determines his position for that variable, in the population being studied. To describe how this intelligence is manifested requires a detailing of specific responses to specific problems. With the techniques used here, the test responses can similarly be considered as a repeated testing of each personality. These responses can be treated additively for the purpose of quantification, and detailed singly for the purpose of illustration.

Psychiatrically, a distinction has been made between those diagnosed as suffering from depression, contrasted with those who were not. This was in effect a distinction between those who were psychotically depressed and the rest who were mainly neurotics, except for a small group of five paraphrenics and paranoid schizophrenics. Here the non-depressed group will be called Group C (N=25), and the psychotically depressed, Group D (N=27). No statistically significant correlation was found between these groups and Groups A and B, the actual distribution being as follows:

	C	D	
A	15	11	26
B	10	16	26
	25	27	

A final comparative assessment was now made between Groups C and D, similar in method to that described for Groups A and B. Common and typical responses are similarly given in Table I under C and D. In this case the Interview successfully distinguished 70 per cent. of the sample, the T.A.T. 74 per cent., and the Rorschach test 85 per cent. After adding the three scores together, a 93 per cent. level of success in differentiation is obtained, and of course the difference in mean scores of the two groups is highly significant. Again, the responses contributing most to these differences will be abstracted and discussed later in the paper.

In both the dichotomies A and B, C and D, then, a high level of differentiation is obtained, demonstrating the usefulness of the method. Each technique also has a fairly high level of differentiation when used by itself, and this level

is roughly the same for the Interview, the T.A.T. and the Rorschach test. The T.A.T., however, takes longer to administer, and is therefore less efficient. An examination of the details of Table I indicates what are the most rewarding topics and test cards. These conclusions may be partly dependent on the type of sample studied here, and results may vary with other groups of records.

In addition to these assessments, calculation of the usual Rorschach scoring categories was made for the sample as a whole, and then between Groups A and B, and between C and D. These results are given in Table II for the whole sample—unless a significant difference was found in either of the dichotomies.

TABLE II

F%	..	..	49.5	W%	..	41.7
F—%	..	..	12.0	D%	..	50.3
				d%	..	4.6
M ..	..	..	1.4	Dd%	..	2.5
FM	..	..	3.3	S%	..	0.9
m ..	..	..	0.8			
EC	..	..	2.9	A%	..	44.0
FC	..	..	1.4	H%	..	10.2
CF	..	..	1.8	At%	..	7.0
C ..	..	..	0.3	Obj%	..	13.2
Last 3%	..	..	38.0	Pl%	..	11.3
Sum c	..	..	1.3	R	..	26.6 (A)
Sum C [†]	..	..	0.3			16.3 (B)
Sum K + k	..	..	1.0			25.6 (C)
						17.7 (D)
				Av.R.T.	..	39.1 secs. (A)
						62.1 secs. (B)
						50.6 secs. (C+D)

RESULTS

I. Characteristics Common in the Group as a Whole

These are responses occurring with a 25 per cent. frequency at least in all the four groupings, A, B, C, and D. To be of a 25 per cent. frequency or more in each of the four groupings means a response will be of at least a 25 per cent. frequency in the sample as a whole. The exact frequency can be obtained from Table I, and frequently reaches a much higher level than 25 per cent.

Interview. In appearance she is of average height and of average build. Her manner is apprehensive, anxious and nervous. Her degree of co-operation may be good or it may be limited. In speech she seems coherent, fluent, and has a reasonable amount of conversation. She says her interests lay mainly in the house and home activities such as sewing and knitting. Her relationships with other people may be very good, or may have difficulties. She prefers being with others to being alone. Her past life may have been reasonable or it may have contained special difficulties. For the future, she wants only to be able to return home and carry on her life as before. Her illness she describes as being one of being apprehensive, anxious and nervous. She feels depressed. A physical complaint of some kind is a prominent component of her complaints. She feels her illness has lasted no more than a few months. She feels her powers of concentration and of remembering are impaired. She claims her will power is strong.

T.A.T. In the absence of adequate norms it is impossible to estimate precisely what is peculiar to this group compared with the normal population.

A comparison with Rosenzweig's (1948) figures, wherever possible, suggests dramatic or violent themes occur more frequently in this group than in the normal population. Similarly, the impression is given that this group is perhaps nearer to Foulds (1953) "hysteric" group rather than his "dysthymic" group. For example, the older figure on (12) is evil and horrible; fear is the common theme with (9), and murder on (13). The child on (7) is here commonly seen as inattentive—implying, perhaps, a fear of children displaying negative attitudes to the mother. Almost certainly important in this group is the specification that (16) should contain children.

Rorschach. The majority of common responses seem to be "popular" or near "popular" ones. An exception perhaps is the whole, colour-form response to IX, with its connotations of poorly controlled affect.

From Table II of the usual Rorschach scoring categories the following seem to distinguish the group as a whole from what are considered "normal" records, and yet do not differ significantly between Groups A, B, C, D: F -per cent., $FM+m>M$, $EC>M$, $CF+C>FC$, W per cent., At per cent. The disruption of form level (F -per cent.) indicates a severity of disturbance more psychotic than neurotic in intensity. There is an inability to come logically to grips with the life situation (W per cent.). This is perhaps because of her poor affective control and tendency to impulsive outbursts ($CF+C>FC$). Her personality is, in fact, dominated by her affective response ($EC>M$), and there is an absence of mature inhibitory processes ($FM+m>M$). A higher than expectancy anatomical content (At per cent.) may indicate her affective response is often of aggressive intensity, is unexpressed and turned back on the self. It certainly suggests hypochondriasis and bodily self-preoccupation.

II. Characteristics Distinguishing Groups A, B, C, D

These are responses occurring with at least a 25 per cent. frequency in any group, and being at least twice the frequency occurring in the other group constituting the dichotomy.

Group A—Interview. She is of average height and of thick-set build. She cries and is distressed during the interview. Her amount of conversation seems reasonable. She says she prefers being by herself to being with others. Her illness, she feels, has been caused by a physical complaint.

T.A.T. She seems to see herself as unhappy, depressed and lonely; (1, 8, 15, 18)—perhaps over her child (18), who is ill—or through thinking over the past (8, 15). She feels middle-aged, or even old (10)—and seeks for comfort from her husband (10). There are further indications of anxiety and apprehension (5, 9)—and perhaps from this stems the suspicion that may be present (9). She presents, perhaps, some opposition to her own parents and to everyday things of the home, in a way suggestive of a common genesis (2). She is more productive and so notes more details of the pictures (5, 11, 17).

Rorschach. There is difficulty with card VII and an attempt is made to objectify and rationalize the problem. However, anxiety is indicated—particularly in respect of the sexual-maternal roles said to be reflected by this card. The red colouring of II causes perplexity and probably parallels a difficulty in dealing with aggressive affect. Conscious concern about the self is indicated in the human response to I. Her greater productivity is seen in the number of "popular" and near "popular" responses that are given.

Group B—Interview. In height she is short. Her degree of co-operation seems limited, as does her amount of conversation.

T.A.T. The violin (1) is perceived as broken, or even misperceived, and together with the sexual misidentification of figures that occurs with (10) and (18), it suggests a breakdown of sexual roles and a loss of sexual identity. There is little overt display of emotional disturbance—for example, the two women on (9) are peacefully together, bathing, or having a picnic. She thinks only of her home (16).

Rorschach. As with Group A, there is difficulty with VII. The rather crude texture response that occurs would be said to reflect feelings of affectional deprivation—particularly in respect of the sexual-maternal roles.

From Table II she is definitely slower (R.T.) and less productive (R.).

Group C—Interview. She says her interests include a sport or sporting activities.

T.A.T. The young woman on (12) is “bad”, and there may be no apparent difficulty here in dealing with blame and guilt. On (18) the figures are a mother and child—the latter may be ill. Perhaps again traumatic themes (or guilt) are avoided by perceiving one of the figures as a man (18).

Rorschach. The whole, colour-form response to IX indicates poorly controlled affect. Although they give a symbolical type of response to VII, there are no signs of other disturbance or free anxiety in this area of sexual-maternal roles. Card IV, however, evokes expressions of surprised distaste, which probably reflect similar feelings towards male figures. The red colouring of II causes perplexity—probably paralleling a difficulty in dealing with aggressive affect.

Group D—Interview. She is short in height, and of thick-set build. She says she feels apprehensive of the future. In describing her illness she is self-blamatory, and expresses ideas of guilt.

T.A.T. There are some signs of affective disturbance in the unhappy, lonely, depressed boy of (1), the unhappy woman of (8), and the “horrible” nature of (15), where the figure is seen as a ghost. But there is really an absence of dramatic themes, and, for example, scenes of quiet domesticity occur (6). Perhaps the lack of clear identification of figures (14), and even sexual misidentification (10) is due to a need to avoid traumatic themes. On (12) it is stressed that the young woman is “good”—it may be the patient identifies herself with the older “bad” woman, contrasting her evil nature with others, and its possible effects on them.

Rorschach. The crude texture response occurring on VII would be said to reflect affectional deprivation—particularly in regard to sexual-maternal roles.

From Table II she is also definitely less responsive.

III. Age, and Intellectual Level

No significant differences were found between Groups A and B, C and D, with regard to age, marital status, or intellectual level. The mean age for the group (which contained 10 spinsters) was 46.5 years. Their intellectual (Progressive Matrices, 1938) and verbal (Mill Hill Vocabulary Scale, Form 1 Senior, 1948) level is in both cases of a mean average grading. More of the sample were of above average ability than below. This was due to the sampling procedure already mentioned, and in any event, was not a statistically significant feature.

DISCUSSION

Two considerations must be made in any discussion of these results. First, the effects of present-day earlier recognition, hospitalization and treatment of psychiatric illness. It may be expected that this has resulted in milder or earlier

forms of the classical diagnostic pictures being seen. Secondly, an investigation of successive admissions such as this entails the inclusion of less clearly defined illnesses than would be utilized in studies of any particular diagnostic grouping.

With this in mind, it seems that those Standard Interview characteristics found to be common in the sample as a whole have many similarities with those included in descriptions of involuntional melancholia.

Thus (cf. Mayer-Gross *et al.*, 1954), they display, and complain of, anxiety and apprehension. They feel depressed. They show hypochondriasis in their physical complaints. Their talkativeness apparently precludes retardation. The apparent restriction of interests and ambitions to the home may also reflect the obsessional, rigid, conscientious features that have been said to characterize the pre-morbid personality (cf. Anderson, 1936; Malamud *et al.*, 1941).

The T.A.T. and Rorschach responses common in the sample help to give a clear expression of their psychological characteristics. There is a consistent interlinking and interweaving of disturbed affect and aggression, hypochondriasis, sexual-maternal capacity, and relationships to children. It is easy to see in this constellation the importance of the social and biological factors usually mentioned in discussions of involuntional melancholia.

Relatively little has been published about Rorschach responses and involuntional melancholia. Frequently, only a passing distinction is made between such a specific syndrome and other psychotic depressions. The data that is available also varies a great deal in quality. From a rough comparison, however, of the data here with that published (Klopfer and Kelley, 1946; Rapaport, 1946, *op. cit.*; Johnson and Sherman, 1948; Kendig and Vernier, 1948; Young, 1950; Kobler and Stiel, 1953), some conclusion can be attempted. There may be some ground for believing that the tendencies here (i.e. higher W per cent., lower F per cent., F— per cent., CF+C>FC, EC>M, At per cent.) reflect some measure of universality.

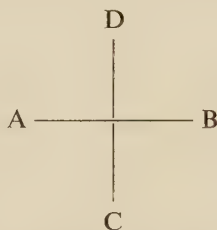
This latter constellation, on examination, has affinities with those described as occurring in hysteria. The dramatic, "hysteric" aspects of the T.A.T. responses have already been noted. These findings seem to parallel observations (Fessler, 1950; Mayer-Gross *et al.*, *op. cit.*) that melancholia may often present behaviour reminiscent of hysteria.

All these characteristics, however, that have resulted from a Standard Interview, the T.A.T. and the Rorschach test are common throughout the sample as a whole. They are not specifically related to either the psychological or psychiatric dichotomies utilized in the groupings A and B, C and D. The conclusion follows that characteristics commonly described as distinguishing involuntional melancholia do not in fact determine a distinct grouping. These features (as detailed in section I of the results) appear to be common to all women presenting a psychiatric illness for the first time during the age range of the biological involution. As the present study is concerned only with women suffering from psychiatric illness, it is impossible to say how common these features are in mentally "normal" women of this age range—or for that matter, in women of other ages in our pattern of culture. As these women have many features in common, clear differentiating characteristics (in the two dichotomies) are limited. The actual sample studied and the techniques utilized may have contributed to this effect. The differentiating characteristics, although real enough, may also, because of their intangible nature, elude the recording framework.

Nevertheless, the psychological characteristics that are typical to Group D (the depressed group) ranging from their physical appearance to their expressed ideas of guilt are fundamental in psychiatric descriptions of depression.

Furthermore, there is no *a priori* reason why either Group B or Group C should present any typical features, as both are only negatively defined by the absence of features constituting Groups A or D respectively. Group C is also known to contain a neurotic and schizophrenic mixture. Both Groups B and C, however, do present some homogeneity.

On examination, in fact, the contrasting features of Groups A and B seem to present something approaching a dichotomy of personality. Perhaps similarities could be drawn between Group B and introversion, and between Group A and extraversion. But a similar parallel can also be made between the contrasting features of D and C and introversion-extraversion. A difficulty now arises because the dichotomy A : B has already been shown to be unrelated to the dichotomy C : D. Could a solution be found by supposing that the dichotomies A : B and C : D were each (or either) only approximations to a single dichotomy of introversion-extraversion?



Thus, in the diagram, the introversion pole would occupy some place between B and D, and the extraversion pole some place between A and C.

Such a solution does not appear possible after paying attention to the exact nature of the relationship between the dichotomies A : B and C : D. Taking a clear example from the T.A.T. For Group D the lonely, unhappy boy on (1) and the two men on (10) are characteristic responses. The former response is also characteristic of Group A, and the latter Group B. With this data, more than one dichotomy or dimension is needed to resolve the distribution. Other examples can be seen on examining the response frequencies in Table I of Groups A, B, C and D. It would seem, therefore, that to describe adequately what is usually subsumed under introversion-extraversion, more than one dichotomy may be required. When this is done, it may become possible to account for many of the peculiar contradictions existing between variously described dichotomies frequently considered to reflect introversion-extraversion. Such an examination is beyond the scope of the present study.

It is interesting to note that much of the content of the features that typify Groups A, B, C and D again seem to reflect the concerns noted as common to the sample as a whole. The actual distinguishing features of the groups seem to lie more in differences in modes of expression rather than in differences of content.

SUMMARY AND CONCLUSIONS

1. The paper presents the psychological part of a study designed in part to re-examine the concept of involutional melancholia. It aimed to estimate more precisely what in fact are the characteristics of women admitted to a mental hospital for the first time during the involutional age range.

2. Fifty-four successive first admissions of women, aged 40-55, formed the sample. Results of a Standard Interview, and in 52 cases, of T.A.T. and Rorschach test results, are presented and discussed.

3. Characteristics hitherto considered consistent with mild or early melancholia appear to be common to all women presenting psychiatric illness for the first time during this age range.

4. These characteristics are not peculiar to either the psychological or the psychiatric dichotomies that were studied.

5. It also seems indicated that fully and correctly to describe the phenomena subsumed under "introversion-extraversion", more than one dichotomy or dimension may be required.

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THE ENDOCRINE CONCOMITANTS OF SCHIZOPHRENIA*

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IN attacking the problem of the relationship of endocrine function to the schizophrenias and other mental diseases, we have avoided the elementary conception that certain hormones are responsible for special psychological traits and have reached the following conclusions. First, the quality of the mental disturbance depends mainly on a genetically conditioned personality pattern, various individuals reacting in different ways to the precipitating causes. Second, the hormones come into the whole picture only in so far as the hormone equilibrium of the body determines how far the individual can adjust himself to the increased demands arising out of the occurrence of various precipitating causes (Reiss, 1955). It is therefore understandable that very many, or even the majority of people suffering from severe endocrine disturbance, need not necessarily show any psychopathological changes, since their personality pattern is not so conditioned and no increased demands for adjustment are made by the occurrence of precipitating causes. On the other hand, it is equally understandable that certain disturbances in hormone production and equilibrium, even when clinically obscure, can be decisive for mental breakdown in individuals with the appropriate personality pattern, at the occurrence of precipitating causes.

We would like the investigations which are now reported to be understood in this way, and in order not to complicate matters we shall describe only some of the most elementary changes in the equilibrium between adrenal, thyroid and pituitary glands, for the moment leaving out the results of more complicated investigations which are still in progress.

Much work already published and based on single investigations on groups of patients leads to contradictory conclusions, even though the results presented appear to have statistical significance. The only absolutely true fact emerging from all such investigations is perhaps best illustrated in Figure 1 where the ordinates are left anonymous.

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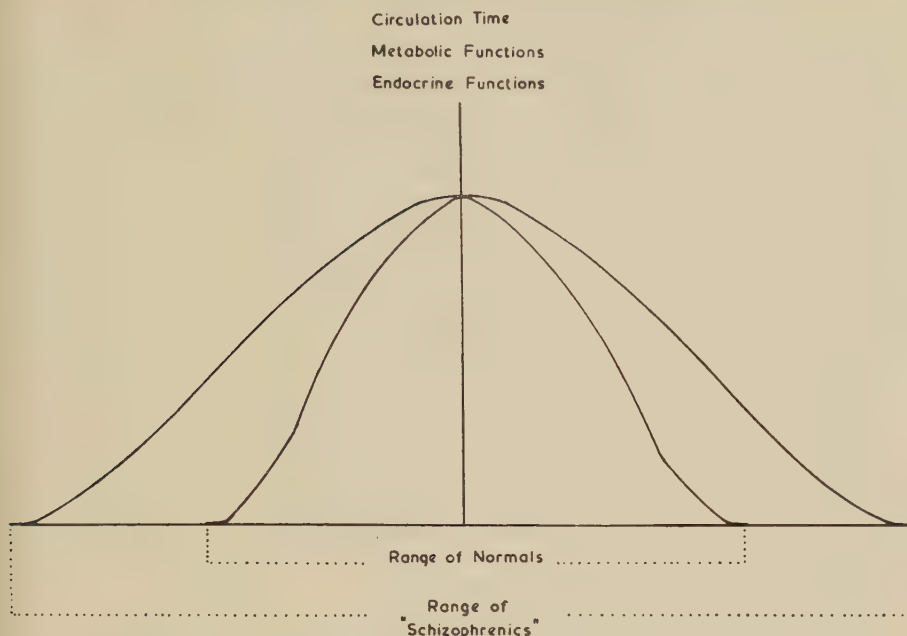


FIG. 1.—Distribution of frequency (vertical axis) of values (horizontal axis) in schizophrenics and non-schizophrenics. Values may be results of single determinations of any parameter, e.g. circulation time, body temperature, basal metabolic rate, blood chemistry, thyroid activity and excretion rate of adrenal cortical hormones. Although schizophrenics and normals may share a common mean value of these parameters, the scatter of values is much wider in schizophrenics than in normals.

This figure can apply to circulation rate, body temperature, basal metabolic rate, blood chemistry, thyroid activity and excretion rate of adrenal cortical hormones. The only fact all these investigations have in common is that the range of the results in schizophrenics is much wider than in normal individuals. In order to obtain reliable results the following approach appears necessary.

1. A better sub-division of clinical types is needed when correlating psychopathological symptoms with biochemical changes. It is not to be expected, for example, that schizophrenics showing great tension should show changes similar to those in quiet, burnt-out schizophrenics.

2. It is necessary to carry out a greater number of determinations on the same patient and to co-ordinate the biochemical and mental state chronologically in the individual case, the technique of "longitudinal section".

3. The investigations should be carried out over a long period of time on a large number of patients and, if possible, using identical techniques, in several hospitals, since certain differences can be seen according to the reception area from which patients are derived.

In this paper, assessments of thyroid function and ketosteroid excretion in schizophrenics are compared with results in non-schizophrenics. Repeated observations, made over varying periods on many patients, are related to changes in mental state and to the results of insulin treatment.

The observations were made on 253 male and 198 female patients in St. Ebba's Hospital, Epsom. Of these, 92 males and 47 females were schizophrenics. For the purposes of this paper, the remaining 161 male and 151 female patients are not grouped according to diagnosis.

INVESTIGATION INTO THYROID FUNCTION

During the past few years it has become customary to use a radioactive tracer method for assaying thyroid activity by measuring the ability of the gland to take up I^{131} . This procedure gives information concerning only one of the functions of the thyroid, namely, the iodine uptake which precedes the production of thyroid hormone, and we have therefore started to determine the protein-bound iodine in blood as a measure of hormone production. In the investigations to be reported here, results based on measurements of I^{131} uptake are discussed, in accordance with the practice of the majority of endocrinologists who regard this as a reliable measure of thyroid activity. In addition, however, since we have to deal with psychiatric patients, other parameters have been measured in order to provide a cross-check of the results and to enable the reliable detection of borderline cases in whom the thyroid activity is beginning to change. These parameters are as follows (Reiss and Haigh, 1954):

1. The uptake slope of I^{131} during the first hour. This is very often indicative of the I^{131} avidity of the thyroid. The steepness of the uptake curve in the first hour often shows an existing over-function. On the other hand, however, absolute flatness of the curve may be due not only to under-function but also to a suppression of the I^{131} uptake by vasoconstrictor substances circulating in the blood during anxiety states.

2. The amount of I^{131} retained in the thyroid (ring count) 24 and 48 hours after the intravenous injection of the tracer. This is measured with a specially constructed ring counter which, placed round the neck of the patient, permits accurate measurement independently of the distance of the thyroid from any part of the counter (Haigh, Reiss and Reiss, 1954).

3. Twenty-four and 48-hour excretion rates of tracer. This figure permits a cross-check since 24-hour uptake (ring count) and excretion rate amount to about 91 per cent. of the total tracer dose.

It has proved useful to adopt an index of the total thyroid activity (I_t) which represents the quotient of uptake rate in the first hour divided by either the I^{131} excretion rate, or the 24-hour I^{131} retention rate of the thyroid.

We established normal ranges by investigating normal control patients and also by statistical means, calculating the arithmetic mean of the values of the whole hospital population and applying the standard deviation on both sides of the arithmetic mean. The normal ranges are in agreement with the normal ranges established in other hospitals. For instance, the 24-hour I^{131} uptake of the thyroid ranges between 25 and 50 per cent. of the injected dose. Of the patients whose 24-hour uptake falls in this range, 90 per cent. have a normal basal metabolic rate. Some cases where a discrepancy exists between B.M.R. and the tracer values will be discussed later on when undersensitivity to thyroid hormone is discussed.

Figure 2 shows the distribution of the thyroid indices in patients in St. Ebba's Hospital. Results in schizophrenics are compared with those in all the other psychiatric cases. It can be seen that as compared with other males, the male schizophrenic patients show a greater frequency of under-function, while female patients do not. This is shown more clearly in Table I.

Our colleagues in the University Psychiatric Clinic at Zürich, using our method, have carried out similar investigations but exclusively on chronic schizophrenic patients (Bleuler and Stoll, 1956). Their results also appear to be in good agreement with the distribution found in the patients of the Bristol Mental Hospitals as already published (Reiss *et al.*, 1951, 1952, 1953).

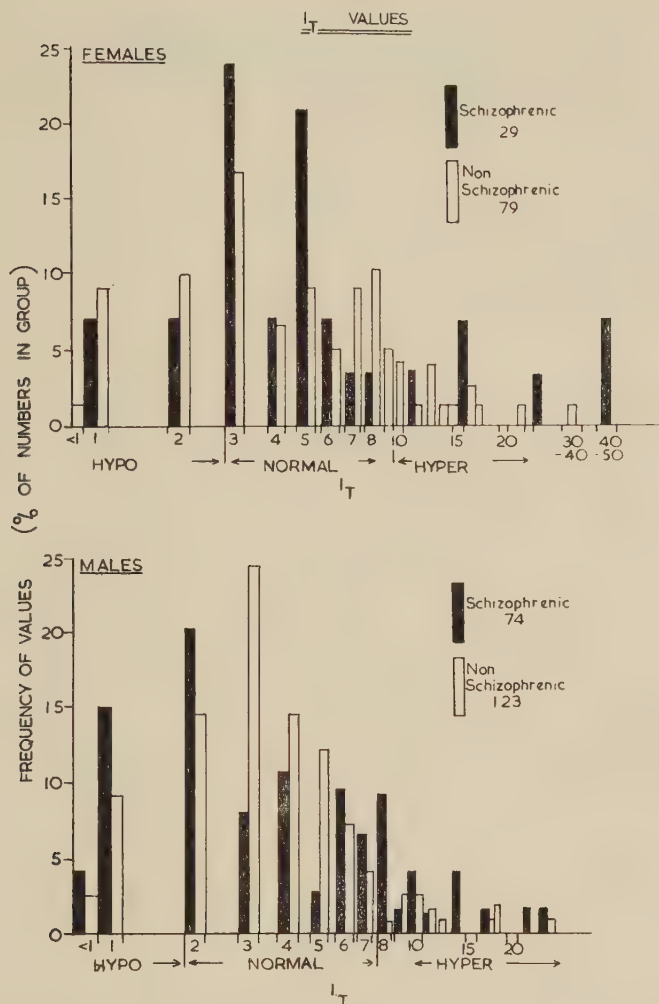


FIG. 2.—Distribution of I_t values (indices of thyroid function) according to sex and mental disorder group. Frequencies of values are expressed as a percentage of number of cases in group.

TABLE I
Thyroid Tests (I_t Values)
Distribution of Results of Tests of Thyroid Function (Thyroid Index, I_t , Reiss and Haigh, 1954) in Schizophrenics and Non-Schizophrenics in St. Ebba's Hospital

			Males		Females	
			Schizo- phrenics Percentage of 74	Non-Schizo- phrenics Percentage of 123	Schizo- phrenics Percentage of 29	Non-Schizo- phrenics Percentage of 79
Normal	..	..	66	79	65	65
Hyper	..	..	15	10	21	15
Hypo	..	..	19	11	14	20

The distribution in male schizophrenics is significantly different from that in male non-schizophrenics.

TABLE II
Thyroid Tests (I_t Values)
Distribution of Results of Tests of Thyroid Function (I_t Values)
in Chronic Schizophrenics in the University Psychiatric Clinic,
Basle

Males			Females
Percentage of 90			Percentage of 73
Normal	..	70	68
Hyper	..	10	25
Hypo	..	20	8

The results are not significantly different from those in Table I.

In 1929, Hoskins and Sleeper found hypothyroidism in the small group of patients they investigated, but they made the error of generalizing. In a way, they made the same observation as Langfeldt (1926), that some schizophrenic patients can take large amounts of thyroid, 30 gr. and more a day, without change in their B.M.R. We have also seen some cases that reacted similarly. We were able to go one step further and identify such cases as showing a peripheral under-sensitivity to thyroid hormone. These patients are not necessarily hypothyrotic. On the contrary, their thyroid activity as assessed by tracer investigation may be normal, or even increased, while their B.M.R. is reduced. Their tissues are not sensitive enough to endogenously or exogenously applied thyroid hormone. This state, which is physiopathologically still unexplained, is seen only in certain cases of pituitary under-function, and otherwise by us mainly in schizophrenia.

In some schizophrenics who show thyroid under-function, the under-function is primary, that is, due to the thyroid itself, but in others the under-function is secondary to a pituitary anterior lobe under-function, as previously reported. These two types of cases can be distinguished by treating them with thyrotrophic hormone. Those patients whose thyroids take up more I^{131} after this treatment suffer from secondary thyroid under-function, while those whose thyroids do not take up more I^{131} after this treatment, are primary hypothyrotics (Reiss and Haigh, 1954).

Cranswick (1955) has lately investigated a group of schizophrenic patients who in a statistically significant number of cases did not react to thyrotrophic hormone. He, therefore, assumes that the thyroid of schizophrenics is not sensitive to thyrotrophic hormone. In the light of our previous discussion, it will be understood why such a generalization is not acceptable.

INVESTIGATIONS INTO ADRENAL CORTICAL FUNCTION

Adrenal cortical function and its disturbances are greatly involved in the pathology of schizophrenia. There are still workers who claim that only under-function of the adrenals occurs in schizophrenia, others only over-function. In our opinion such a division of opinion is futile. Not only under-function and over-function but also completely normal function have been seen by us in schizophrenia. In fact, the situation is very similar to that which has just been discussed in relation to thyroid function.

Not only 17-ketosteroids were investigated as a measure of adrenal function, but also various ketosteroid fractions and corticoids. In this paper, however, for simplicity discussion is restricted to the 17-ketosteroid excretion rate in a

schizophrenic hospital population, and to a correlation of the 17-ketosteroid excretion rate with spontaneous changes in the psychiatric disease and with changes seen after treatment. In the whole schizophrenic population at St. Ebba's Hospital the range of 17-ketosteroid excretion is considerably wider than the normal range generally accepted in the literature (7-18 mg./24 hours). A survey made on 75 schizophrenics over the age of 20 in St. Ebba's showed a range of ketosteroid excretion between 2 and 48 mg. per day: Lingyaerde (1953) similarly reports a very much wider range of excretion in psychiatric patients as compared with controls. In more than 40 per cent. of his patients the excretion rate was below or above the normal range. Figure 3 shows the excretion rate of our schizophrenics as compared with other psychiatric patients, according to age range.

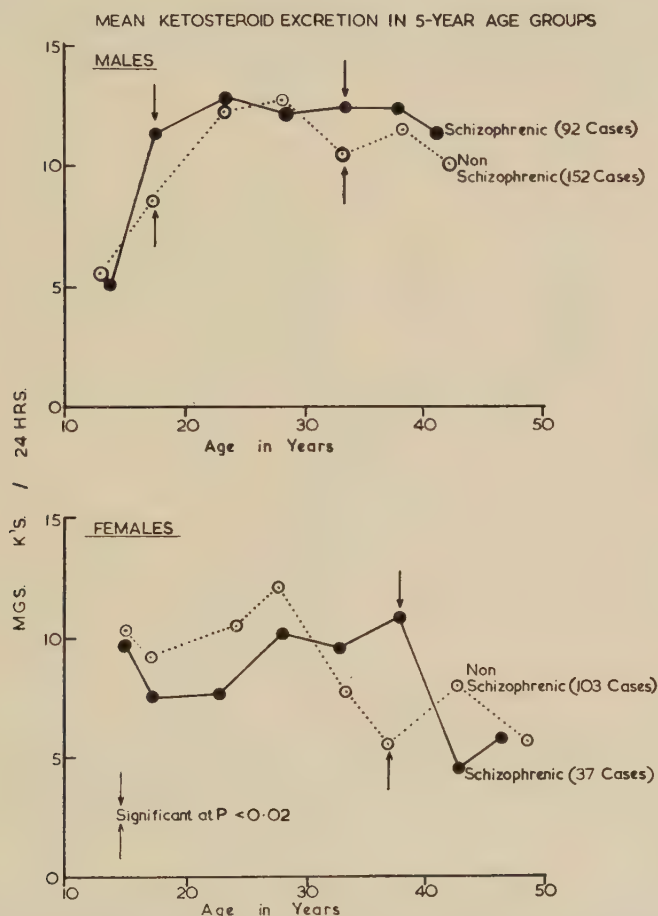


FIG. 3.—Mean 17-ketosteroid excretion rates of schizophrenics and non-schizophrenics according to sex and mean age in quinquennial age groups. The arrows indicate excretion rates that are statistically significantly different when means in the same quinquennium are compared.

Paranoid schizophrenics show a statistically significantly higher ketosteroid excretion rate (Table III). These findings might well be linked with our clinical observation that paranoic women show hypertrichosis more frequently than do other schizophrenics.

TABLE III
Paranoid and Non-Paranoid Schizophrenics

	17-ketosteroid Excretion, mg./24 Hours					
	Cases	Range	Mean	S.D.	<i>t</i>	<i>P</i>
Paranoid schizophrenics . .	19	7.3-27.3	16.8	5.4	4.16	0.001
Non-paranoid schizophrenics	45	1.2-26.5	10.1	5.9		

The correlation of a patient's mental state with his ketosteroid excretion rate over long periods has proved to be very informative. Such long-term investigations on individual cases might contribute more to our understanding of the significance of adrenal cortical function in schizophrenia than will the statistical evaluations of single results from groups of cases. Figure 4 shows an example where this was done. There is a distinct association between the more disturbed aggressive phases of a 30-year-old male, and his ketosteroid excretion level.

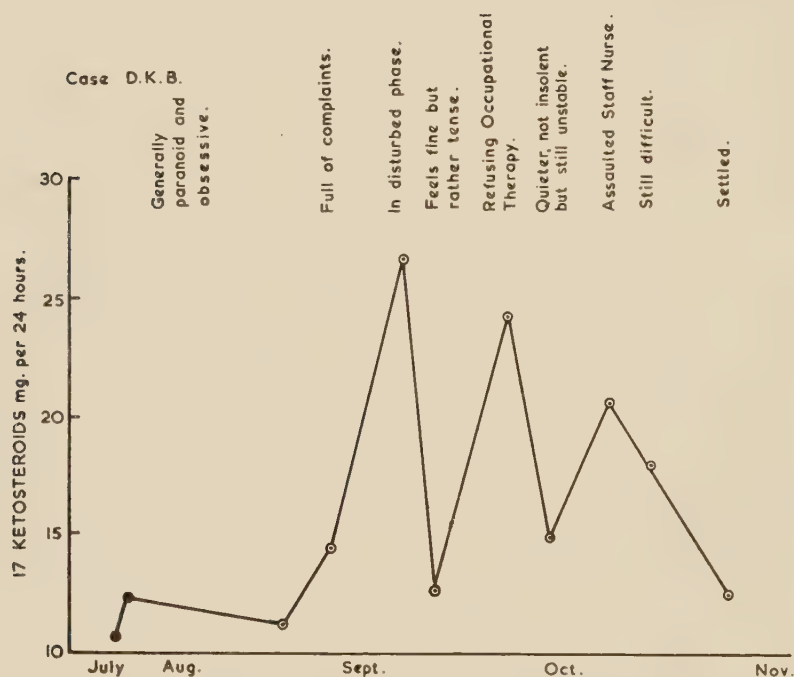


FIG. 4.—Correlation of changes in mental state and in 17-ketosteroid excretion rate. Male schizophrenic, age 30. No treatment during the period of observation. Generally, peaks of excretion were associated with difficult and sometimes aggressive behaviour whilst low excretion rates were associated with quieter behaviour. Thyroid activity: raised (R.C. 62 per cent.) at beginning, becoming normal (R.C. 51 per cent. end of September).

One of the most rewarding results of such correlation was first seen in some cases where the psychiatrist withheld specific treatment to see what a change of environment alone could achieve for the patient when he first came under hospital care and attention. Some of these patients showed a spontaneous remission, and it was most interesting to see how a normal hormone equilibrium,

including ketosteroid excretion rate and thyroid function, became restored, sometimes even before the mental improvement was finally recognized. Various forms of changes in hormone equilibrium during such spontaneous remissions can be seen in Figure 5. Such investigations are at the moment carried out

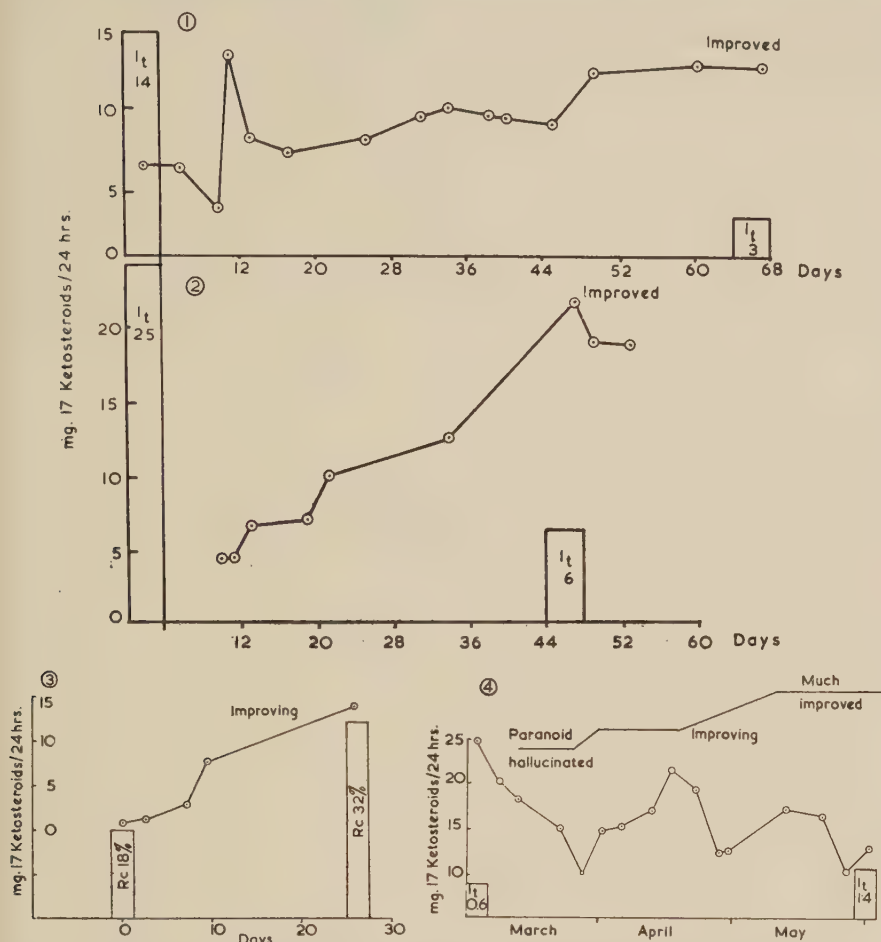


FIG. 5.—17-ketosteroid excretion rates in 4 cases of schizophrenia showing “spontaneous” improvement (i.e. effect of hospitalization without physical treatment). In all cases the tendency is for abnormal excretion rates to become normal. Thyroid activity abnormal at first, also becomes normal. (I_t —thyroid index. R.C.=ring count 24 hours after injection of tracer dose of I^{131} .)

systematically on newly admitted acute patients by various members of our research group. It might well happen in time to come that whenever this normalizing tendency in the function of the thyroid and adrenal cortex is found, it will be possible to advise the psychiatrist to withhold physical treatment, as the patient would be likely to recover spontaneously. A normal hormone equilibrium already existing at the time of admission of the patient to hospital might also quite rightly delay physical treatment, in order to determine if the patient is on the way to spontaneous recovery. Again, a period of observation of the kind described might be useful to some extent in differentiating between a really severe psychopathological state and a mere slight exaggeration of a

pre-existing personality pattern that will quieten down under hospital care and attention only.

The investigation of the relationship of changes in ketosteroid excretion rate and in thyroid function to the clinical course of events in insulin coma therapy has led to the recognition of the following different types of reaction. Figure 6 shows tracings of ketosteroid excretion during the course of deep

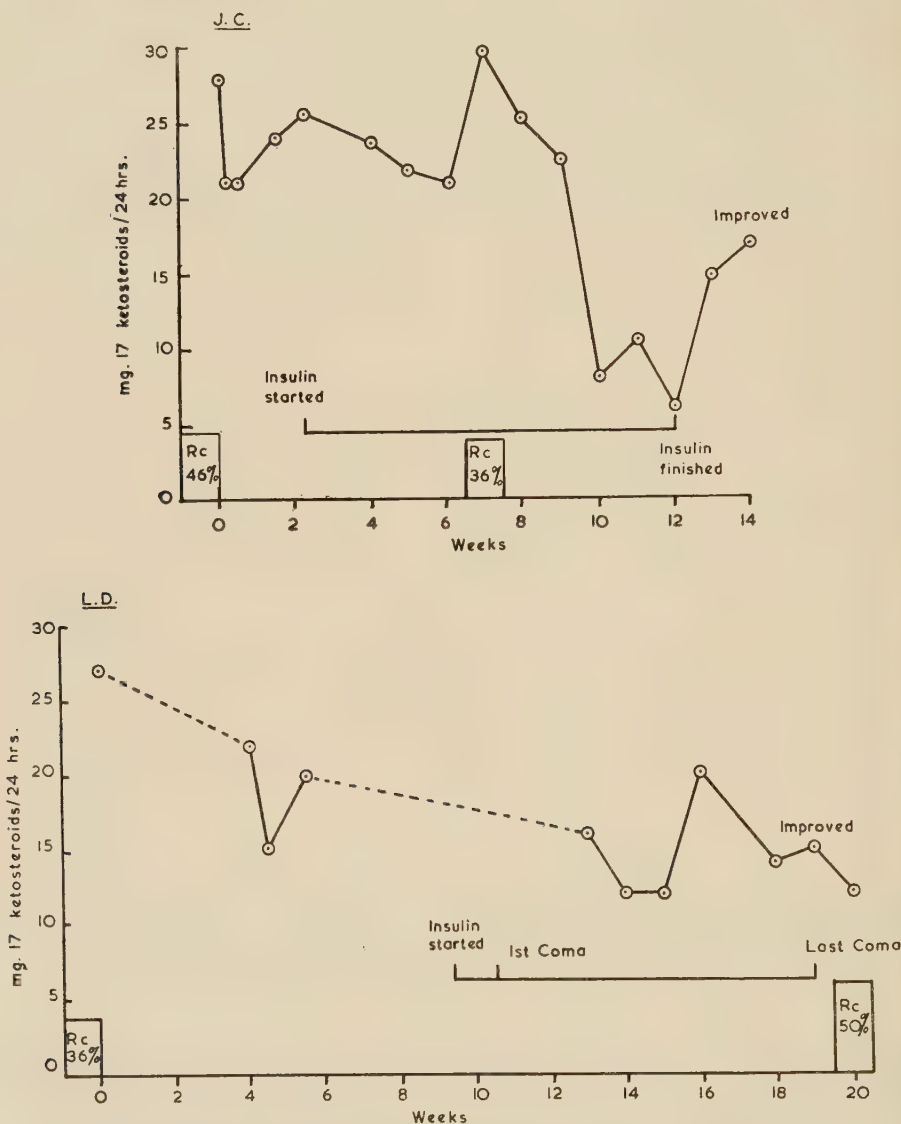


FIG. 6.—17-ketosteroid excretion rates in insulin coma therapy. Type (i) The “standard” type of curve in successful therapy. Male schizophrenics: J.C. 29 years of age, L.D. 24 years of age. Note the fall in excretion with final level in normal range. Thyroid function (R.C. = ring count at 24 hours after injection of tracer dose of I^{131}) normal in both cases but showed favourable changes.

insulin coma therapy and afterwards, in two patients who improved under this treatment. This type of curve may be described as the standard curve to be seen

in really successful insulin coma therapy. Before treatment, the ketosteroid excretion rate was very high, in the upper normal range, or sometimes considerably above it. During insulin treatment the ketosteroid excretion rate—sometimes after a short rise—begins to go down continuously, falling to low levels, occasionally even below the normal range. At this time a certain mental improvement usually occurs. Afterwards the ketosteroid excretion rate remains in the middle normal range. Concomitant with these changes, thyroid function, low at first, increases but remains in the normal range. The patient is much improved and fit for discharge. There are, however, other patients, whose excretion curves are seen in Figure 7, who start exactly as those previously

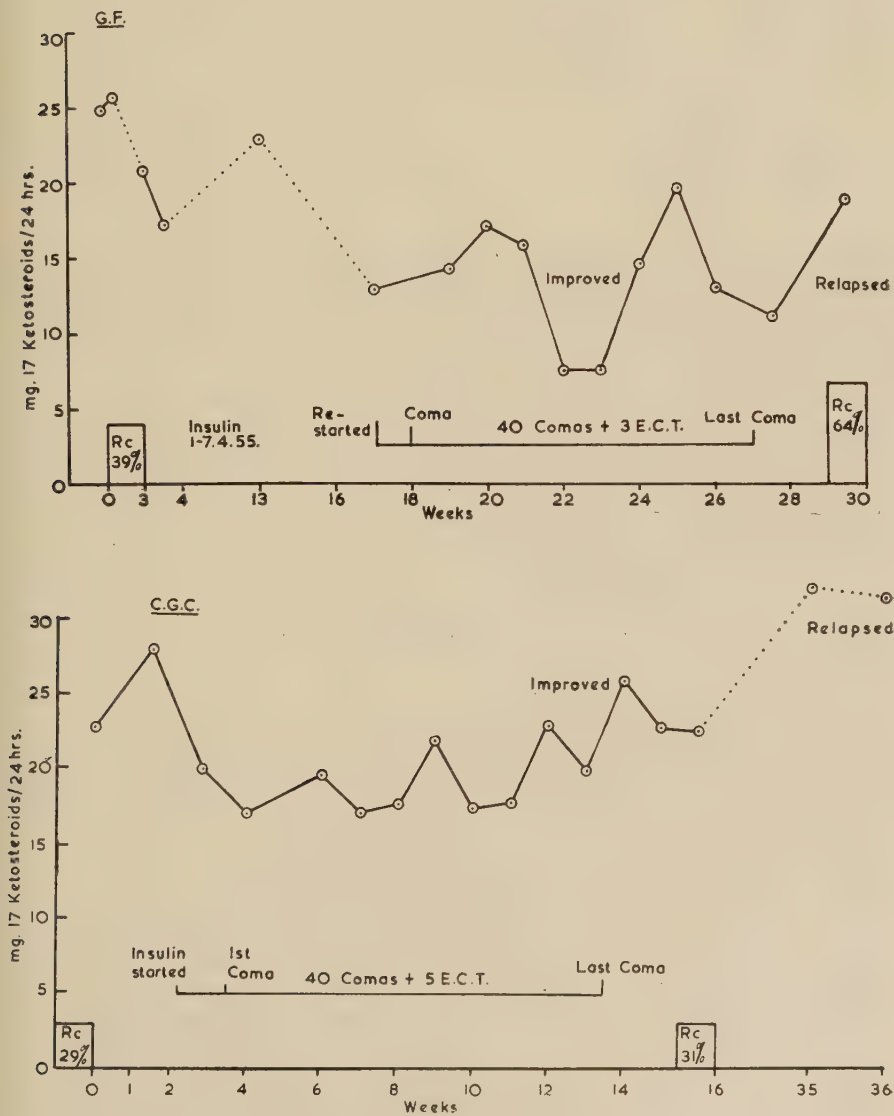


FIG. 7 (continued overleaf.)

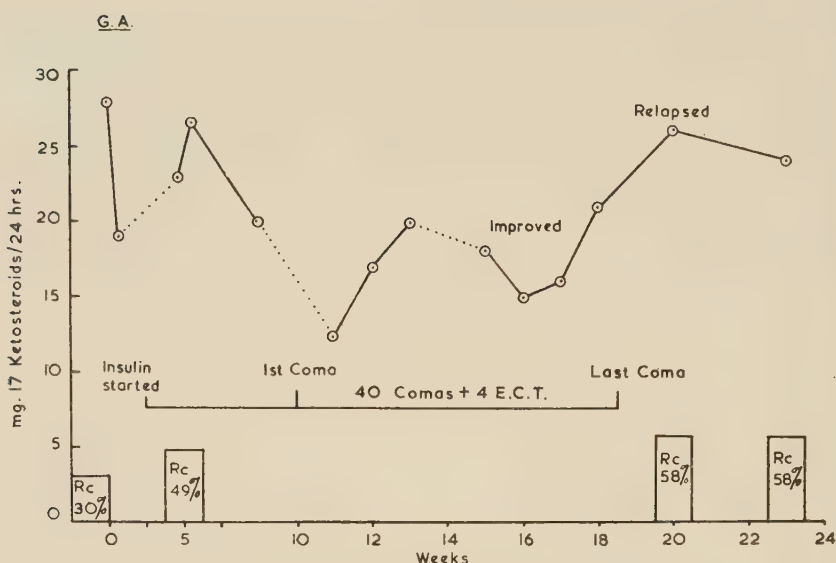


FIG. 7.—17-ketosteroid excretion rates in insulin coma therapy. Type (ii) Temporary improvement with relapse after cessation of treatment. Male schizophrenics: G.F. 25, C.G.C. 19 and G.A. 26 years of age. In all cases the fall in the excretion rates during the middle of the treatment is followed by an increase into the higher normal range or even above. Thyroid function is either not improved (C.G.C.) or passes into range of hyper-function (G.F. and G.A.).

mentioned. When the ketosteroid excretion rate becomes low, some mental improvement is noted and is still present when the ketosteroid excretion later shows a rising tendency. At this stage the patient may even be discharged as much improved. In these cases, thyroid function, low at first, remains low or increases above the normal range. If the ketosteroids rise still further the patient relapses and cannot be discharged, or, if discharged, relapses and returns to hospital in a comparatively short time. In several cases, such a course of events was predicted from the laboratory, to the great surprise of the psychiatrist. One might well assume that these two types of patients had originally a hyper-function of the adrenal cortex, which became exhausted under the continuous stress of insulin coma treatment.

There are, however, other schizophrenics who, to start with, have a very low ketosteroid excretion rate. These are mainly younger patients with retarded development. In only very few of them can the stress of insulin coma therapy raise their ketosteroid excretion rate into the normal range, as seen in Figure 8, a process which is accompanied by mental improvement. In these patients, thyroid function, if normal at first, remains normal, or if abnormal at first, becomes normal. A considerable number of these patients, however, show changes in the ketosteroid excretion rate, as shown in Figure 9. For a short time at first insulin coma therapy can raise the ketosteroid excretion rate, but very soon this falls again below the original level, while thyroid function either remains unchanged, or may go up above the normal range. Mental improvement is never seen in such patients.

Figure 10 shows ketosteroid excretion curves during and after insulin treatment of patients, who, before treatment, had a ketosteroid excretion rate in the normal range. The results of treatment in these cases are very capricious. Sands thought that some patients of this type might have recovered spontaneously without insulin coma treatment. During treatment the first patient shows

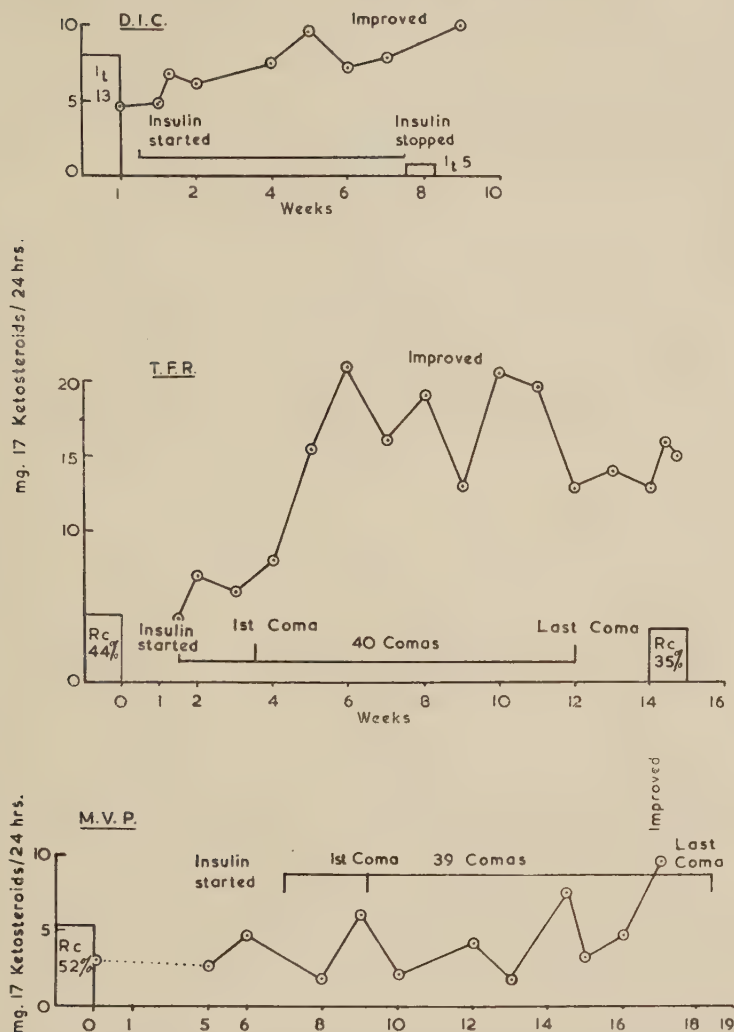


FIG. 8.—17-ketosteroid excretion rates in insulin coma therapy. Type (iii) Treatment successful in patients with initially low excretion rates. Male schizophrenics: D.I.C. aged 15, T.F.R. aged 30, M.V.P. aged 17. The 17-ketosteroid excretion rate increases into the normal range. Thyroid function where normal (T.F.R. and M.V.P.) remains normal or where abnormal (D.I.C.) becomes normal.

great fluctuations in the excretion rate. Towards the end of his treatment, however, excretion is in the normal range. The thyroid activity in this patient was above the normal range before treatment and in the normal range at the end of treatment. This patient showed a good improvement. Another patient, however, who also before treatment showed a normal ketosteroid excretion rate, after completion of the treatment had a ketosteroid excretion rate below the normal range. His thyroid activity was above the normal range before treatment and remained so after treatment. This patient did not improve.

Our investigations so far seem to indicate that for improvement to occur by insulin coma treatment, it is essential that the ketosteroid excretion rate and thyroid activity should both come into and remain in the normal range. If one

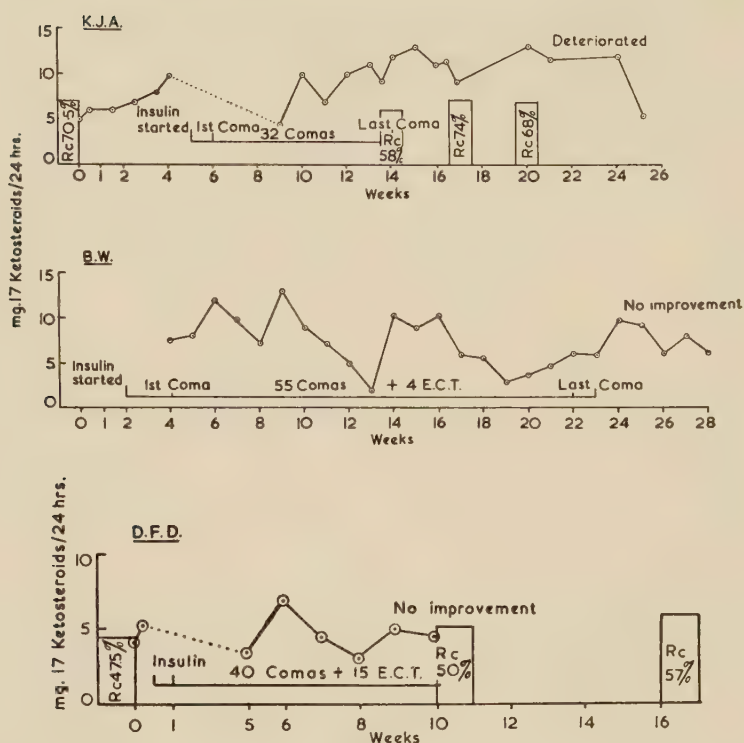


FIG. 9.—17-ketosteroid excretion rate in insulin coma therapy. Type (iv) Treatment unsuccessful. Schizophrenics: K.J.A. male, aged 15, B.W. female, aged 22, D.F.D. male, aged 35. Excretion, below normal or in low normal range at beginning, not improved. Thyroid function where normal (B.W. and D.F.D.) remained normal (B.W.) or moved to hyper-normal range (D.F.D.), or where abnormally high remained so (K.J.A.).

of these values is found, at the end of treatment, to be outside the normal range a relapse can be predicted, even if the patient is so well at the time that he can be discharged from hospital.

We conclude, therefore, that the results of these initial investigations of the changes in the adrenal cortex-thyroid equilibrium form a tentative basis for predicting the therapeutic result of insulin coma treatment and of predicting a relapse after treatment. Attempts at prognosis can also be supported by various clinical endocrinological observations. Patients who show high ketosteroid excretion rates, also usually show well developed gonads, sometimes even prematurely developed genitalia and secondary hair distribution, with particularly dense hair on the thighs, and a well-developed beard. In these, the prognosis for a good result from insulin coma therapy is more favourable than in others. Patients with ketosteroid excretion rate below the normal range are often sexually retarded. They have comparatively small testicles, with no cremaster reflex, very little secondary hair and no beard at the age of 19, but the most striking factor is often an open inguinal canal. In these the prognosis for insulin coma therapy is often unfavourable. Although open inguinal canal occurs quite frequently in the normal population, it can always be regarded as an objective sign of a certain degree of immaturity. The testicles need not by any means always be cryptorchic, but there is a possibility that on certain occasions, particularly when the patient lies down for his night's sleep, the testicles are

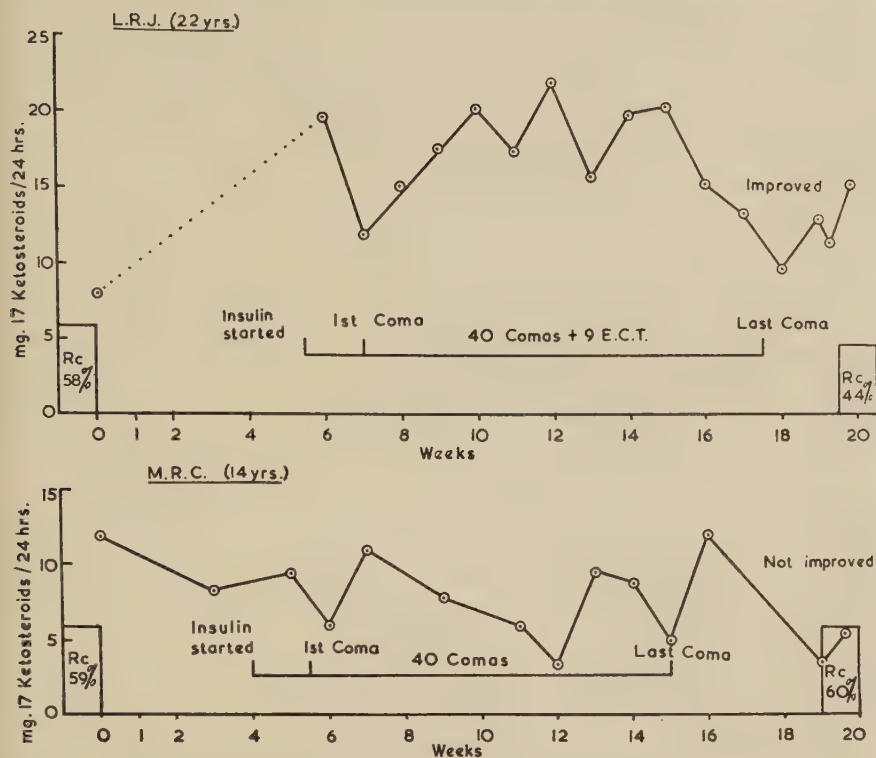


FIG. 10.—17-ketosteroid excretion rates in insulin coma therapy. Type (v) Initial excretion in normal range. Male schizophrenics: L.R.J. aged 22. Excretion at first raised, then normalized. Thyroid function initially high became normal. This patient improved with the treatment. M.R.C. aged 14. Excretion tends to be depressed as treatment progresses. Thyroid function above normal and not changed. No improvement.

retracted into the abdomen. Such a process is certainly unfavourable for the normal development and function of the testicles. During our investigations we have not seen one schizophrenic, who had an open inguinal canal, improve after insulin coma therapy. This agrees with the clinical observations of Sands, that insulin coma therapy was unsuccessful in 80 per cent. of his juvenile cases when it was carried out before a certain degree of sexual maturity had been reached.

There is still another state to differentiate. During clinical investigation young men are found not infrequently with a high ketosteroid excretion rate and well developed genitals but with no trace of a beard and no hair on the chest. These are patients whose tissues are insensitive to sex hormone. This under-sensitivity is similar to the under-sensitivity to thyroid hormone mentioned before. The mechanism of peripheral under-sensitivity to a hormone is not yet clear. Gordan (1955) quotes a number of endocrinologists who regarded the disturbed sensitivity to thyroid hormone as a symptom of hypopituitarism.

PITUITARY ANTERIOR LOBE FUNCTION

It is a considerable drawback in the endocrine analysis of schizophrenia that very little work has been devoted to the direct investigation of pituitary anterior lobe functions. This is due to the fact that methods for investigating anterior pituitary lobe hormones in body fluids are not yet sufficiently developed

to be used as routine research methods. However, our results to date make it quite certain that a primary disturbance of pituitary anterior lobe function in the frame of the neuro-endocrine interrelation is very important. In schizophrenics, thyroid, adrenal and gonadal function can be disturbed simultaneously, a state which can only be due to a disturbance of the function of the pituitary anterior lobe in producing the relevant trophic hormones. It is possible for the production of one trophic hormone to be reduced while others are over-produced, thus resulting in various states of disturbed hormone equilibrium. For instance, there may be increased thyroid activity and decreased adrenal cortical activity, or simultaneous increase of thyroid and adrenal cortical activity and so on.

Many workers nowadays regard schizophrenia as a maladaptation to stress. The role of the anterior pituitary lobe in neuro-endocrine interrelations during stress is well known. Intensive study of individual young male acute schizophrenics, their social background, employment, manner of life, make it seem probable that the high ketosteroid excretion rate often connected with a certain degree of pre-maturity, is the result of intermittent stress conditions, and represents a form of adaptation. These patients come, mainly, from metropolitan reception areas. There is a group of schizophrenics of opposite type with very low ketosteroid excretion rate who are often retarded in their development and subject to continuous stress conditions in various aspects. Their low ketosteroid excretion rate might well be the expression of a state of endocrine exhaustion.

In this connection, it should be noted that in the majority of puerperal cases, with varying psychiatric symptoms, either a very high or a very low ketosteroid excretion rate is found, apparently according to the range of adaptation of the pituitary anterior lobe and adrenal cortex in each individual.

When discussing pituitary anterior lobe function, it is necessary also to consider disturbances in gonadal function in relation to the production of gonadotrophic hormone, since the occurrence of gonadal disturbances in some schizophrenics is known. An association between masturbation and schizophrenia has long been in the minds of some psychiatrists. In the standard psychiatric text-book of Clouston, in 1898, masturbation was even regarded as a cause of what would now be called catatonic schizophrenia. Today it is well established that sexual activity stimulates increased production of gonadotrophic hormone; equally, so can masturbation. It can therefore be regarded as feasible that frequent stress, or the frequent masturbation often seen in schizophrenics could lead to some degree of over-stimulation or exhaustion of the pituitary anterior lobe, and in turn respectively to sexual prematurity, or the arrest of sexual development at various stages.

DISCUSSION

When discussing the results just described, it is necessary to avoid some obvious misunderstandings, which can occur either in analysing primary cause and secondary effect or in the expectation from hormone therapy in psychiatry. If psycho-endocrine research is to become useful in treating mental disease, basic conceptions must be carefully formulated. In any patient, an endocrine disturbance may obviously be primary or secondary in the development of the disease. The neuro-endocrine vicious pathological circle must be continually kept in mind. The precipitation of an acute disorder might be compared with the development of an avalanche, which starts with a slight movement in the snow, and ends by uprooting or destroying all in its path. The results are seen, but not the primary cause. Similarly in mental disease, owing to neuro-endocrine interrelationships it is to be expected that there are always some endocrine

disturbances accompanying mental disease, even though the initiating cause may be obscure. In fact, it would not be surprising, as investigation methods improve, to find that every acute recent case always showed some abnormal endocrine function.

Hormone therapy should never be regarded as intended to deal with the primary cause, but merely as an attempt to improve a disturbed homoeostasis and enable the patient either alone or with the help of the psychotherapist to adjust himself to the demands of his environment.

Reviewing the results reported in this paper it cannot be doubted any longer that in some schizophrenics endocrine disturbance plays an important role. Thyroid under-function and over-function, as well as normal function, can occur. Claims of positive results of treatment from the thyroid aspect reported in the literature may certainly be correct. We ourselves have some experiences in this direction based, however, on proper indications after appropriate investigation of the patients. The same things can be said about adrenal cortical function.

The results of our investigations into the action of insulin coma treatment are particularly illuminating, and help to put the role of the adrenal cortex in schizophrenia in a better perspective. The success of insulin therapy in patients having a high ketosteroid excretion rate initially, seems consistent with the improvement gained by Allen and Broster (1945) after adrenalectomy in psychiatric patients, mostly paranoid, with hyperadrenalism. Other facts can make this picture even more nearly complete: for example, the administration of cortisone or A.C.T.H. is known to produce psychotic symptoms—apparently in individuals so disposed; treatment with high doses of dehydroisoandrosterone sometimes resulted in aggressiveness and schizophrenic symptoms. In both cases the symptoms disappeared after cessation of treatment. Finally, it must be realized that during puberty, and particularly during adrenarche, huge amounts of ketosteroids are being poured into the blood stream, a state which might be termed physiological hyperadrenalism.

In recent acute cases after definite stress, where there is a low ketosteroid excretion rate, it may not be necessary to give insulin coma treatment, even though several such cases in our group did well on it. This, of course, agrees with clinical experience. Equally, if the patient has a low ketosteroid output with an insidious development of illness, it is usually of little use to give insulin. Finally, one can learn from the changes seen after insulin coma therapy that improvement in the ketosteroid excretion rate alone is not enough, but that also the thyroid function should be normalized in cases where insulin treatment is to prove beneficial.

SUMMARY

1. Variations in thyroid activity in schizophrenics are described. Schizophrenia can be accompanied by normal, under- or over-function of the thyroid.
2. The 17-ketosteroid excretion rate in schizophrenics varies over a wider range than is found in the normal population. As with the thyroid, the adrenal cortex may show normal, under- or over-function in schizophrenia.
3. The correlation of changes in the 17-ketosteroid excretion rate with changes in the mental state has been attempted in individual patients.
4. Paranoid schizophrenics have a statistically significantly higher 17-ketosteroid excretion rate than do other schizophrenics.
5. Changes in adrenal cortical and thyroid function during spontaneous change of mental state have been studied. Normalization of function of both glands is an essential concomitant of spontaneous improvement.
6. Various types of changes in 17-ketosteroid excretion rate and in thyroid function found during insulin coma treatment, have been correlated with therapeutic results.
7. Conclusions concerning the significance of the adrenal cortex in the schizophrenic state are stated and discussed.

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We wish to express our grateful thanks to all who have collaborated with us, doctors, biochemists, nurses and laboratory and office staffs. We are also grateful to the South-west Metropolitan Regional Hospital Board for a research grant for biochemical work.

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BIOCHEMICAL STUDIES IN ELECTRIC CONVULSIVE THERAPY

By

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FOR this purpose average normal values were studied in 40 normal cases, and 40 mental cases were selected from amongst those who were suffering from various mental disorders who were given electric convulsive therapy.

The biochemical investigations included estimation of blood sugar, plasma proteins, blood urea, N.P.N., uric acid, serum potassium, plasma sodium and plasma chloride.

These estimations were carried out before, immediately after, 2 hours after, 4 hours after, 24 hours after and after 10 electric shocks (after 4 days).

The average normal values of the blood chemistry were observed in the 40 normal cases who were mostly students and doctors of this medical college.

Their age groups were as follows:

TABLE I

Maximum age	..	..	..	45 years
Minimum age	..	..	..	17 years
Average age	..	..	..	25 years

And their blood chemistry was as follows:

TABLE II

	Maximum	Minimum	Average
Blood sugar mg./100 c.c.	120	70	93.9
Plasma proteins g./100 c.c.	7.8	5.2	6.52
Blood urea mg./100 c.c.	39.4	17.5	26.1
Blood N.P.N. mg./100 c.c.	34.5	16.5	23.1
Plasma uric acid mg./100 c.c.	4.2	2.0	3.0
Serum potassium mg./100 c.c.	20.6	15.6	17.6
Plasma sodium mg./100 c.c.	340	314	327.0
Plasma chloride mg./100 c.c.	390	335	358.5

Forty mental cases who were selected for this study belonged to the Mental Hospital, Agra. These belonged to the following age groups (see Table below).

TABLE III

Age in Years	No. of Cases	Per cent. of Cases
1. 11-20	11	27.5
2. 21-30	20	50
3. 31-40	4	10
4. 41-above	5	12.5

The disease which they were suffering from belonged to the following groups:

TABLE IV

Diseases				No. of Cases	Per cent. of Cases
1.	Schizophrenia	..	..	17	42
2.	Manic reaction type	..	..	10	25
3.	Manic depressive psychosis	..	..	6	15
4.	Melancholia	..	..	3	7.5
5.	Psychoneurosis	..	..	2	5
6.	Paranoid	..	..	1	2.5
7.	Confusional insanity	..	..	1	2.5

BLOOD SUGAR

The changes which occurred in the blood-sugar level (mg./100 c.c. of blood) before and after the E.C.T. were as follows:

TABLE V

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	118	142	124	112	115	120
Minimum value	70	76	69	69	75	75
Average value	88.3	117.3	100.6	100.6	92.9	96.9

TABLE VI

Blood Sugar mg./100 c.c.	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
65- 69 ..	—	—	1 (2.5%)	—	—	—
70- 80 ..	11 (27.5%)	1 (2.5%)	5 (12.5%)	10 (25%)	7 (17.5%)	6 (15%)
81- 90 ..	12 (30%)	0	7 (17.5%)	10 (25%)	12 (30%)	6 (15%)
91-100 ..	7 (17.5%)	4 (10%)	5 (12.5%)	11 (27.5%)	11 (27.5%)	16 (40%)
101-110 ..	4 (10%)	5 (12.5%)	9 (22.5%)	8 (20%)	4 (10%)	4 (10%)
111-120 ..	6 (15%)	12 (30%)	11 (27.5%)	1 (2.5%)	6 (15%)	8 (20%)
121-130 ..	—	11 (27.5%)	2 (5%)	—	—	—
131-140 ..	—	5 (12.5%)	—	—	—	—
141-150 ..	—	2 (5%)	—	—	—	—

Table VII shows the abnormal blood-sugar levels in the number of cases thus studied.

TABLE VII

	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	39 (95%)	28 (70%)	21 (52.5%)	21 (52.5%)	32 (80%)
No change ..	—	2 (5%)	7 (17.5%)	17 (42.5%)	8 (20%)
Fall	1 (2.5%)	10 (25%)	12 (30%)	2 (5%)	—

It was observed from these estimations that immediately after the E.C.T. the blood-sugar value increased in all the cases as shown in the Table VII. In one it fell down by 9 mg. The increase varied from 14 mg. to 55 mg. with an average rise of 21.3 mg. After two hours of the E.C.T. the blood-sugar level showed a tendency towards a decline. In some cases it went below the initial level (Table VII). The maximum rise after two hours was 47.5 mg. and the maximum decrease 8 mg.

In 7 cases the blood sugar rose from that level which was obtained immediately after the shock while in 33 cases it came down.

After 4 hours of the E.C.T. the blood sugar further declined towards the initial level and in some cases even below it. There was a decrease in 38 cases from the value obtained immediately after shock while in one case it increased and in one it was equal.

Comparing these figures with the figures obtained after two hours of the shock it was noticed that in 7 cases (17.5 per cent.) the value showed a rise, in 6 cases (15 per cent.) it was equal and in the remaining it fell down.

After 24 hours of E.C.T. the blood-sugar level was higher in 21 cases (52.5 per cent.) as compared with the initial level and lower in 2 cases only. In one case this value was higher than the value obtained after E.C.T.

After 10 shocks the blood-sugar level was higher in 32 cases (80 per cent.) as compared with the initial reading. In the remaining 8 cases (20 per cent.) it was equal and in none was it less.

Conclusion. From this study we find that immediately after E.C.T. there is a transient hyperglycaemia. This hyperglycaemia gradually declined and reached the initial levels in the next 24 hours. After multiple shocks a slight rise in blood-sugar level was noticed.

PLASMA PROTEINS

The changes occurring in the plasma proteins (g./100 c.c.) before and after E.C.T. were as follows:

TABLE VIII

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	7.2	8.0	7.4	7.0	7.4	7.4
Minimum value	5.2	5.7	5.2	5.0	5.3	5.5
Average value	6.21	6.86	6.19	6.03	6.24	6.34

TABLE IX

Proteins g./100 c.c.	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
5 -5.5 ..	5 (12.5%)	—	7 (17.5%)	7 (17.5%)	5 (12.5%)	3 (7.5%)
5.51-6 ..	11 (27.5%)	4 (10%)	13 (32.5%)	15 (37.5%)	11 (27.5%)	7 (17.5%)
6.11-6.5 ..	13 (32.5%)	7 (17.5%)	9 (22.5%)	10 (25%)	12 (30%)	15 (37.5%)
6.51-7 ..	7 (17.5%)	15 (37.5%)	9 (22.5%)	8 (20%)	10 (25%)	10 (25%)
7.11-7.5 ..	4 (10%)	8 (20%)	2 (5%)	—	2 (5%)	5 (12.5%)
7.51-8 ..	—	6 (15%)	—	—	—	—

Table X shows the abnormal plasma-protein level in the cases thus studied.

TABLE X

			Immedi- ately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	..	..	38 (95%)	18 (45%)	3 (7·5%)	18 (45%)	34 (85%)
No change		..	—	7 (17·5%)	9 (22·5%)	12 (30%)	3 (7·5%)
Fall	..	..	2 (5%)	15 (37·5%)	28 (70%)	10 (25%)	3 (7·5%)

It was observed from these estimations that immediately after the electric shock the plasma-proteins level increased in all the cases except 2 (5 per cent.) in which there was a fall, the fall being up to -0.3 g. from the initial level. The rise varied from 0.2 g. to 1.35 g.

After two hours of the E.C.T. the plasma proteins showed a tendency to decline. In some cases the decrease occurred even below the initial levels. The maximum rise at this stage was 0.3 g. and fall -0.6 g. The values were lower in all the cases when compared to the values obtained immediately after shock.

After 4 hours of the E.C.T. in 70 per cent. cases the values were lower than the initial level, in 7.5 per cent higher and in the rest equal. The deviation from the initial level varied from 0.2 to -0.95 g. After 24 hours of the E.C.T. the deviation from the initial figures varied from $+0.3$ g. to -0.92 g. After 10 electric shocks the plasma-protein level varied from 0.6 g. to -0.3 g. as compared to the initial reading, while in the rest it was either equal or increased.

Conclusion. From this study we find that hyperproteinaemia occurred immediately after the E.C.T., which came down to the initial level during the next 2-4 hours. During the next 24 hours the values showed no gross change. After multiple shocks a slight rise in plasma-protein level was observed.

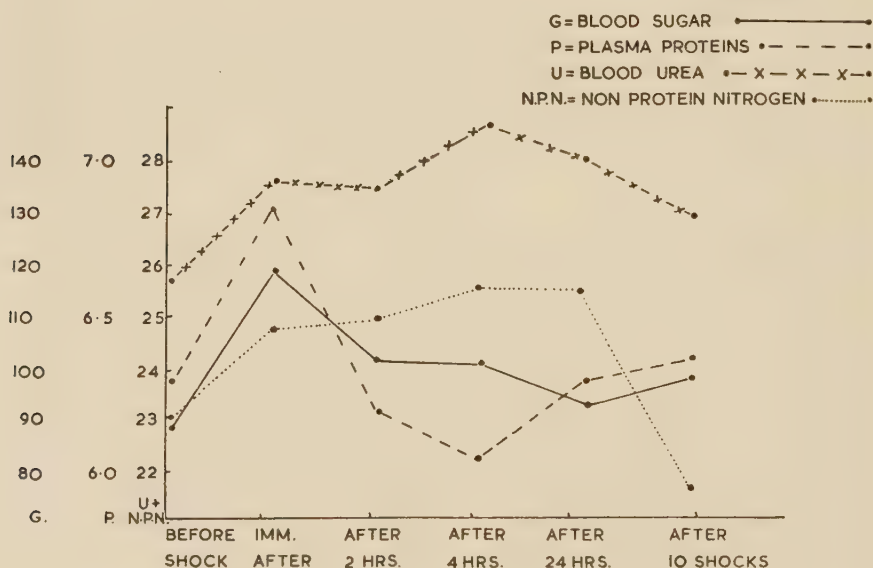


FIG. 1.

BLOOD UREA

The changes which occurred in the blood-urea level before and after the E.C.T. were as follows:

TABLE XI

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	33.6	37.5	28.9	38.9	37.2	38.9
Minimum value	20.0	19.5	21.9	22.5	20.8	19.5
Average value	25.5	27.5	27.4	28.6	29.0	26.9

TABLE XII

Blood Urea	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
18 -20 ..	2 (5%)	2 (5%)	—	—	—	1 (2.5%)
20.1-25 ..	21 (52.5%)	13 (32.5%)	13 (32.5%)	9 (22.5%)	12 (30%)	15 (37.5%)
25.1-30 ..	10 (25%)	12 (30%)	15 (37.5%)	17 (42.5%)	14 (35%)	14 (35%)
30.1-35 ..	5 (12.5%)	8 (20%)	9 (22.5%)	11 (27.5%)	10 (25%)	6 (15%)
35.1-40 ..	2 (5%)	5 (12.5%)	3 (7.5%)	3 (7.5%)	4 (10%)	4 (10%)

Table XIII shows the abnormal blood-urea levels in these cases.

TABLE XIII

	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	31 (77.5%)	34 (85%)	38 (95%)	34 (85%)	29 (72.5%)
No change ..	6 (15%)	4 (10%)	2 (5%)	4 (10%)	3 (7.5%)
Fall	3 (7.5%)	2 (5%)	—	2 (5%)	8 (20%)

It was found out from these estimations that immediately after E.C.T. the blood-urea level showed a rise in most cases (see above Table). The deviation from the initial level varied from 5.3 mg. to -2.4 mg./100 c.c.

After two hours of the electric shock the blood urea varied from 4.6 mg. to -2.4 mg. as compared to the initial reading. The figures when compared to that obtained immediately after the shock showed a rise in 16 cases (40 per cent.), fall in 17 cases (42.5 per cent.) and no change in the other 7 cases (17.5 per cent.).

After 4 hours of E.C.T. there was found a rise in the blood-urea level in all cases except two in which it remained equal to the initial level, the rise varied up to 6.2 mg.

After 24 hours of the E.C.T. the variations ranged from 7.5 to -1.7 mg./100 c.c. Compared to the level obtained after 4 hours there was found a rise in 15 cases (37.5 per cent.), fall in 19 cases (47.5 per cent.) and no change in the rest.

After 10 electric shocks the blood-urea level varied from $+8.8$ to -4.2 mg./100 c.c., as compared to the initial level and on the whole was very slightly higher.

Conclusion. It was found that there was a slight rise in the blood-urea level immediately after shocks which persisted in 85 per cent. cases to a slight degree even after 24 hours. After multiple shocks blood-urea level was found to be very slightly higher, about 1 mg. on an average.

N.P.N.

The changes which occurred in the blood N.P.N. before and after the E.C.T. were as follows:

TABLE XIV

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	32.0	35.0	34.9	35.0	33.5	34.9
Minimum value	18.0	17.6	19.7	20.3	18.7	17.6
Average value	22.9	24.7	24.8	25.5	25.5	21.6

TABLE XV

N.P.N.	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
15 -20 ..	9 (22.5%)	4 (10%)	1 (2.5%)	—	3 (7.5%)	2 (5%)
20.1-25 ..	21 (52.5%)	19 (47.5%)	24 (60%)	21 (52.5%)	18 (45%)	23 (57.5%)
25.1-30 ..	7 (17.5%)	12 (30%)	12 (30%)	14 (35%)	11 (27.5%)	8 (20%)
30.1-35 ..	3 (7.5%)	5 (12.5%)	3 (7.5%)	5 (12.5%)	8 (20%)	7 (17.5%)

Table XVI shows the abnormal blood-N.P.N. levels in the cases thus studied.

TABLE XVI

	After Shock	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	31 (77.5%)	34 (85%)	38 (95%)	34 (85%)	39 (72.5%)
No change ..	6 (15%)	4 (10%)	2 (5%)	4 (10%)	3 (7.5%)
Fall	3 (7.5%)	2 (5%)	—	2 (5%)	8 (20%)

It was found from these estimations that immediately after E.C.T. the blood-N.P.N. level showed a rise in most cases (Table XVII). The deviation from the initial level varied from 4.8 to -2.1 mg./100 c.c.

After two hours of the electric shocks the blood-N.P.N. level varied from 4.0 to -3.0 mg. as compared with the initial reading. The figures when compared with that obtained immediately after the shock showed a rise in 17 cases (42.5 per cent.), fall in 17 cases (42.5 per cent.), and no change in the remaining 6 cases (15 per cent.).

After 4 hours of E.C.T. the blood-N.P.N. showed further rise in most cases. In no case was it found below the initial level. The rise from the initial level varied up to 5.6 mg./100 c.c. When these figures were compared with those obtained immediately after the shock, they were found higher in 26 cases (65 per cent.), equal in 4 cases (10 per cent.), and lower in 10 cases (25 per cent.). The figures compared to the reading after 2 hours were higher in 26 cases (65 per cent.), equal in 11 cases (27.5 per cent.), and low only in 3 cases (7.5 per cent.).

After 24 hours of E.C.T. the variations ranged from +6.8 to -1.6 mg./100 c.c. On the whole there occurred a fall in the values as compared to the values obtained after the shock.

After 10 shocks the deviations from the initial level ranged from 9.5 to -3.5 mg. The blood-N.P.N. level was very slightly higher in most cases.

Conclusion. From this study we found that there was a rise in blood-N.P.N. after E.C.T. which persisted in some degrees even after 24 hours. After 10 electric shocks 72.5 per cent. of cases were found to have a slightly higher blood-N.P.N. level.

PLASMA URIC ACID

The changes which occurred in the plasma-uric acid level (mg./100 c.c.) before and after the E.C.T. were as follows:

TABLE XVII

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	4.2	4.5	5.1	5.0	4.9	4.3
Minimum value	2.0	2.1	2.4	2.4	2.1	2.0
Average value	2.9	3.2	3.7	3.8	3.4	3.1

TABLE XVIII

Plasma Uric Acid mg./100 c.c.	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
2 -2.5 ..	8 (20%)	8 (20%)	1 (2.5%)	1 (2.5%)	5 (12.5%)	8 (20%)
2.6-3 ..	14 (35%)	8 (20%)	5 (12.5%)	5 (12.5%)	5 (12.5%)	12 (30%)
3.1-3.5 ..	8 (20%)	13 (32.5%)	7 (17.5%)	6 (15%)	13 (32.5%)	13 (32.5%)
3.6-4 ..	9 (22.5%)	8 (20%)	16 (40%)	17 (42.5%)	11 (27.5%)	6 (15%)
4.1-4.5 ..	1 (2.5%)	3 (7.5%)	6 (15%)	4 (10%)	3 (7.5%)	1 (2.5%)
4.6-5 ..	—	—	4 (10%)	7 (17.5%)	3 (7.5%)	—
5.1-5.6 ..	—	—	1 (2.5%)	—	—	—

Table XIX shows the abnormal plasma-uric acid levels in the cases thus studied.

TABLE XIX

	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	30 (75%)	39 (97.5%)	39 (97.5%)	32 (80%)	23 (57.5%)
No change ..	—	1 (2.5%)	1 (2.5%)	6 (15%)	8 (20%)
Fall	10 (25%)	—	—	2 (5%)	9 (22.5%)

It was found from these estimations that immediately after E.C.T. there occurs a marked variation in the uric acid level, a rise occurred in 75 per cent. of cases and a fall in 25 per cent. The deviations varied from $+0.6$ to -0.6 mg./100 c.c. as compared to the initial reading.

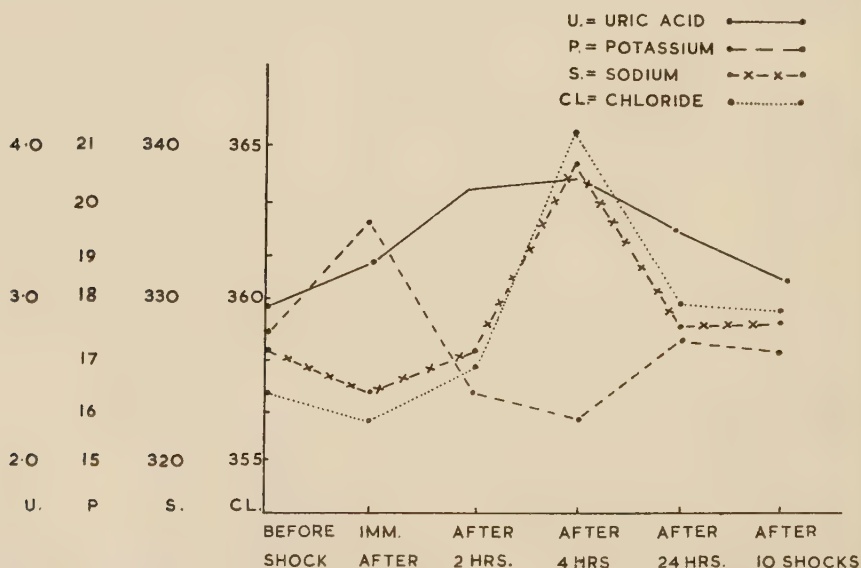
After two hours of the E.C.T. there occurred a further rise in all the cases except one, the rise going up to 1.4 mg./100 c.c. In no cases was a fall observed as compared to the previous reading.

After 4 hours of E.C.T. the maximum rise was 1.3 mg./100 c.c. The values were higher in all cases when compared with the initial readings. As compared with the reading after 2 hours there was found a rise in 18 cases (45 per cent.), fall in 9 cases (22.5 per cent.), and no change in the rest of the cases.

After 24 hours of E.C.T. there was found a slight fall in the uric acid-level which now varied from $+1.2$ to -0.2 mg. as compared with the initial reading.

After 10 shocks the deviations from the initial levels varied from $+1.0$ to -0.8 mg./100 c.c., being higher in 23 cases (57.5 per cent.) and lower in 22.5 per cent. of cases.

Conclusion. It was found that a rise in plasma-uric acid level occurred during E.C.T. which was maximum between 2-4 hours after the shock and persisted in some cases up to 24 hours. After multiple shocks the plasma-uric acid increased very slightly.



FIG

SERUM POTASSIUM

The changes occurring in the serum potassium (mg./100 c.c.) before and after the E.C.T. were as follows:

TABLE XX

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	20.0	21.8	19.2	18.4	19.6	19.4
Minimum value	15.2	16.0	14.0	13.4	15.0	15.0
Average value	17.2	19.3	16.2	15.7	17.2	17.0

TABLE XXI

Serum Potassium mg./100 c.c.	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
13-15 ..	—	—	10 (25%)	15 (37.5%)	1 (2.5%)	3 (7.5%)
15.1-16 ..	8 (20%)	1 (2.5%)	8 (20%)	8 (20%)	9 (22.5%)	9 (22.5%)
16.1-17 ..	11 (27.5%)	5 (12.5%)	10 (25%)	11 (27.5%)	11 (27.5%)	12 (30%)
17.1-18 ..	10 (25%)	6 (15%)	6 (15%)	5 (12.5%)	8 (20%)	7 (17.5%)
18.1-19 ..	5 (12.5%)	11 (27.5%)	5 (12.5%)	1 (2.5%)	7 (17.5%)	7 (17.5%)
19.1-20 ..	6 (15%)	4 (10%)	1 (2.5%)	—	4 (10%)	2 (5%)
20.1-21 ..	—	5 (12.5%)	—	—	—	—
21.1 to above ..	—	8 (20%)	—	—	—	—

Table XXII shows the abnormal serum-potassium levels in the cases thus studied.

TABLE XXII

	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	37 (92.5%)	4 (10%)	—	5 (12.5%)	9 (22.5%)
No change ..	—	2 (5%)	1 (2.5%)	19 (47.5%)	7 (17.5%)
Fall	3 (7.5%)	34 (85%)	39 (97.5%)	16 (40%)	24 (60%)

It was observed from these estimations that immediately after the shock there occurred a rise in serum potassium in most cases (Table XXII). The deviation from the initial level varied from +3.2 mg. to -1.4 mg. After two hours of the shock there occurred a fall in most cases (87.5 per cent.) as compared to the level obtained immediately after the shock.

The deviation from the initial level varied from 1.4 to -2.4 mg.

After 4 hours of the E.C.T. it was found that in all cases except one in which no change was seen, the serum-potassium level fell and was below the initial values, the fall varying up to -2.8 mg. The fall was minimum at this phase except in 2 cases (5 per cent.) in which the level had slightly risen from the values obtained after two hours of shock.

After 24 hours of shock there was a rise in the serum-potassium level from the level obtained after 4 hours of the shock. The deviations from the initial level varied from $+0.8$ to -0.2 mg.

After 10 shocks the deviation from the initial level varied from $+1.2$ to -2 mg., the average level being slightly lower.

Conclusion. From this study we find that immediately after the E.C.T. there occurred a slight rise in serum-potassium level. During the next 2-4 hours the process reversed and in nearly all the cases there was a fall. After 24 hours the level came up to near the initial level. After 10 shocks the values obtained were slightly lower in most cases.

PLASMA SODIUM

The changes occurring in the plasma sodium (mg./100 c.c.) before and after the E.C.T. were as follows:

TABLE XXIII

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	338	345	340	348	341	338
Minimum value	360	308	310	322	314	316
Average value	327.4	324.6	326.8	338.3	328.4	328.4

TABLE XXIV

Plasma Sodium	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
305-310 ..	—	4 (10%)	2 (5%)	—	—	—
311-315 ..	—	7 (17.5%)	1 (2.5%)	—	1 (2.5%)	—
316-320 ..	9 (22.5%)	6 (15%)	8 (20%)	—	4 (10%)	6 (15%)
321-325 ..	7 (17.5%)	4 (10%)	5 (12.5%)	1 (2.5%)	8 (20%)	5 (12.5%)
326-330 ..	9 (22.5%)	9 (22.5%)	11 (27.5%)	4 (10%)	11 (27.5%)	15 (37.5%)
336-340 ..	4 (10%)	6 (15%)	7 (17.5%)	13 (32.5%)	3 (7.5%)	4 (10%)
341-above ..	—	3 (7.5%)	—	14 (35%)	1 (2.5%)	—

The following Table shows the number of cases which showed a deviation from the initial plasma-sodium level:

TABLE XXV

	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise	10 (25%)	17 (42.5%)	40 (100%)	24 (60%)	23 (57.5%)
No change ..	6 (15%)	4 (10%)	—	8 (20%)	5 (12.5%)
Fall	24 (60%)	19 (47.5%)	—	8 (20%)	12 (30%)

It was observed from these observations that immediately after E.C.T. there occurred a fall in the plasma-sodium level in most of the cases (Table

XXV). The deviation from the initial level varied from +12 to -14 mg. in different cases. After 2 hours of E.C.T. the value showed a tendency to return to the initial levels but still the extremes varied from +10 to -12. As compared to the readings obtained immediately after shock there occurred a rise in 25 cases (62·5 per cent.) and a fall in 12 cases (30 per cent.).

In all cases, after 4 hours of E.C.T. the plasma-sodium level was higher than the initial figures and reached the maximum value the range being up to 18 mg.

After 24 hours of E.C.T. there was seen a decline in the plasma-sodium level, it being lower than the level obtained after 4 hours of shock in 97·5 per cent. of cases and equal in 2·5 per cent. As compared to the initial level it was lower in 20 per cent. of cases.

After 10 shocks the deviations from the initial level varied from +8 to -7 mg. In 57·5 per cent. of cases it was higher than the initial level and lower in 30 per cent. The average value was very slightly higher than the initial value.

Conclusion. It was seen that immediately after the E.C.T. there occurred a slight fall in plasma-sodium level. After 2-4 hours of shock the process reversed and a rise in the plasma level occurred which came down to the initial levels during the next 24 hours. After 10 shocks no marked change occurred. The values remained within the extremes of normal limits at all times.

PLASMA CHLORIDE

The changes which occurred in the plasma-chloride level (mg./100 c.c.) before and after the E.C.T. were as follows:

TABLE XXVI

	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Maximum value	390	390	385	405	490	390
Minimum value	340	320	320	350	335	335
Average value	357·1	356·0	357·5	365·5	359·7	359·6

TABLE XXVII

Plasma Chloride mg./100 c.c.	Before Shock	Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
320-335 ..	—	5 (12·5%)	5 (12·5%)	—	1 (2·5%)	1 (2·5%)
336-340 ..	9 (22·5%)	4 (10%)	3 (7·5%)	—	3 (7·5%)	3 (7·5%)
341-350 ..	6 (15%)	7 (17·5%)	9 (22·5%)	1 (2·5%)	8 (20%)	10 (25%)
351-360 ..	9 (22·5%)	13 (32·5%)	7 (17·5%)	8 (20%)	9 (22·5%)	10 (25%)
361-370 ..	7 (17·5%)	7 (17·5%)	9 (22·5%)	6 (15%)	7 (17·5%)	8 (20%)
371-380 ..	6 (15%)	3 (7·5%)	5 (12·5%)	9 (22·5%)	9 (22·5%)	6 (15%)
381-390 ..	3 (7·5%)	1 (2·5%)	2 (5%)	9 (22·5%)	3 (7·5%)	2 (5%)
391-405 ..	—	—	—	7 (17·5%)	—	—

Table XXVIII shows the abnormal plasma-chloride levels in the cases thus studied.

TABLE XXVIII

		Immediately After	After 2 Hours	After 4 Hours	After 24 Hours	After 10 Shocks
Rise		8 (20%)	11 (27·5%)	40 (100%)	17 (42·5%)	15 (37·5%)
No change		8 (20%)	10 (25%)	—	18 (45%)	12 (30%)
Fall		24 (60%)	19 (47·5%)	—	5 (12·5%)	13 (32·5%)

It was found from these estimations that immediately after E.C.T. there occurred a fall in the plasma level of chloride in most cases (Table XXVIII). The deviations from the initial level varied from +20 to -25 mg./100 c.c. in different cases.

After two hours of E.C.T. the value had a tendency to return to the initial levels but still the extremes varied from +10 to -20 mg./100 c.c. As compared to the readings obtained immediately after shock there occurred a rise in 22 cases (55 per cent.), a fall in 13 cases (32·5 per cent), and in the remaining 5 cases (12·5 per cent.) no change.

In all cases after 4 hours of E.C.T. the plasma chloride showed a fresh rise and reached maximum value, the range varying up to 30 mg. from the initial values.

After 24 hours of E.C.T. there was seen a decline in plasma-chloride level, it being lower than the level obtained 4 hours after the shock. The figures came down to near about the initial reading with a variation of +15 to -15 mg./100 c.c.

After 10 shocks the deviation from the initial level varied from +10 to -10 mg./100 c.c., the average level being very slightly higher than the initial values.

Conclusion. From this we find that immediately after the shock there occurred a fall in the plasma-chloride level. After 2-4 hours the process reversed and there occurred a slight rise in its level which came down to the initial level during the next 24 hours. After 10 shocks there occurred a very slight rise in the plasma-chloride level. The values remained within the extremes of normal limit in nearly all the readings.

OBSERVATIONS

1. Patients who suffered from the various types of mental disorders did not show any change in the biochemical findings before the electric convulsive therapy was started nor did they show any deviation from the normal cases.

2. There is an immediate transient hyperglycaemia after electric convulsive therapy. The rise in blood sugar gradually declines and in about 4 hours it reaches practically the normal level. After 10 electric shocks have been given slight rise in blood sugar occurs.

3. There is transient hyperproteinaemia occurring after E.C.T. This rise begins to decline and reaches the initial level in 2-4 hours. After multiple shocks a slight rise is maintained for some days.

4. After E.C.T. there is only slight rise in blood urea and non-protein nitrogen which even persisted for 24 hours in some cases. After multiple shocks the blood urea and N.P.N. do not show any marked change in their blood levels.

5. There is a rise in the plasma-uric acid level which reaches its maximum in 2-4 hours and the normal level in 24 hours. After multiple shocks there is only a slight rise in some cases.

6. A rise in serum potassium occurs immediately after the shock and then it declines in 2-4 hours and reaches even below the initial figures in the next 24 hours. There is no change in this after multiple shocks.

7. Plasma sodium chloride falls immediately after the shock. It slowly rises above the initial value in 2-4 hours time and then again declines to its normal value in 24 hours time. Multiple shocks do not lead to any change.

8. It was observed from these estimations that there was a close relationship between blood urea and N.P.N. A rise in one accompanied rise in the other and vice versa.

9. Immediately after the convulsions there was a transient increase in all (plasma proteins, urea, N.P.N. and uric acid) the above constituents in most cases. After two to four hours it was seen that though the level of plasma protein came down to about the initial level, the level of others remained at the higher levels and an increase in them persisted in most cases and then decreased, but was still higher than the initial levels even after 24 hours.

10. It was observed that in nearly all the cases a rise in plasma sodium was followed by a rise in plasma chloride and a fall in serum potassium and vice versa.

CONCLUSIONS

From the above observations we can make out that there is an immediate rise of the blood-sugar proteins, urea, N.P.N., etc.

The rise in the blood urea, N.P.N. and uric acid may be due to the increased protein metabolism during the convulsions.

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RECOVERY TIME FROM MODIFIED AND UNMODIFIED E.C.T.

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DURING the last few years the use of electroconvulsive therapy in the treatment of out-patients has greatly increased. For example, in 1951, one hundred and forty-one treatments were given in the out-patient department of Freedom Fields Hospital, Plymouth, while in 1955, four hundred and sixty-four were given. There is no doubt that the number of treatments given to out-patients has increased even more in the country as a whole, as many hospitals did not start giving E.C.T. on an out-patient basis until well after 1951.

With this great increase in the number of patients treated it becomes, of course, the more imperative to select the method which is the safest, most effective, and economical in the time of both staff and patients. Four methods were considered in the present investigation:

1. The use of unmodified E.C.T., producing a full grand mal seizure.
2. The application of the same electrical stimulus ten to fifteen seconds after the beginning of fasciculation induced by the intravenous injection of suxamethonium chloride ("Scoline") (20-50 mg. according to weight). (The use of relaxants without anaesthesia was first tried out by Gillie and McNeill (1), and further work was done by Kelleher and Whiteley (2).)
3. The intravenous injection of a sleep dose of thiopentone sodium ("Pentothal") (150-300 mg. according to weight) immediately followed by a dose of suxamethonium chloride (20-50 mg. according to weight), preparatory to the electrical stimulus.
4. The use of a similar technique as in (3), but using buthalitone sodium ("Transithal") (250-500 mg.) as suggested by McColl (3) in a letter to the *Lancet*.

In all cases, patients were given a premedication of 1/75th gr. subcutaneous atropine.

The design of the investigation was as follows. Patients were an unselected group, and consisted of all those attending the out-patient E.C.T. clinic over the period of the experiment. They ranged in age from 20 to 70 and both sexes were represented. Patients were given their atropine injection on arrival at the Clinic. The treatment was then given about half-an-hour later, in one of the four ways described, and at the moment the shock was administered a stop-watch was started. The times when the patient finally stopped twitching and when he drew his first breath were then noted. As soon as he was sufficiently recovered he was handed over to the care of a nurse who kept him under close observation and noted the time at which he regained consciousness as judged by his ability to respond to simple commands.

Several criticisms may be levelled at this method, although they do not appear to invalidate it. In the first place the question of judgment on the part of the individual nurse might well arise, and, secondly, it might be argued that it was essential for the same nurse to be used on each occasion, to reduce any observer error. In practice this proved impossible for administrative reasons, and from time to time the nurses taking part in the investigation were changed. However, in spite of this, it was found that different nurses gave very similar times for recovery so long as one particular method was being used. It was also felt that, as the series was a large one, minor observer errors would tend to cancel out. Another factor which was not specifically controlled was age, but although very old or debilitated patients occasionally show a delayed recovery, this was by no means the rule, and they formed such a small part of a large series that any such delay would have no significant effect on the final result.

For convenience of description the various phases of the sequelae of the electrical shock were numbered. Phase one represents the time between the stimulus and the cessation of twitching. Phase two is the time between the stimulus and the first breath. Phase three is the time between the stimulus and the recovery of consciousness as judged by the criterion mentioned above.

Before tabulating the findings it is worth describing briefly the pattern of response to each method of treatment, and considering relevant problems of technique. First will be discussed unmodified or "straight" E.C.T. In this, the patient immediately upon stimulation goes into clonic spasm which is succeeded by tonic movements during which there is apnoea, and cyanosis develops. In almost every case breathing starts at the same instant as the relaxation of spasm.

If, however, scoline was given intravenously before the shock, the picture was quite different. It was found that the rapidity of action of scoline varied considerably from patient to patient, so that it was found best to judge the best time to apply the stimulus on the effect of the scoline on the individual patient, rather than to stick rigidly to a time-table. The shock was given as soon as fasciculation became well established in the face or upper limbs, or as soon as the patient began to show signs of discomfort (whichever was the earlier). The actual figures varied between 10 and 30 seconds. When this method was used the convulsion was markedly modified and a fairly prolonged period of apnoea succeeded the cessation of the spasm, so that insufflation with oxygen was always necessary.

When the anaesthetic was injected intravenously just before the scoline, it was found advisable to delay the stimulus until fasciculation was well established all over the body and beginning to pass off, in order to ensure the greatest degree of modification. Again, the time between injection and stimulation varied, but worked out on an average thirteen seconds longer than when scoline alone was used. With pentothal a prolonged period of apnoea succeeded the cessation of the twitching, so that insufflation was necessary in nearly every case. However, using transithal, the period of apnoea was very short indeed, and breathing usually recommenced only a few seconds after the cessation of the twitching. With transithal, insufflation was rarely essential.

As the full findings were striking and somewhat complex, it would be best to present them in tabular form, and then to discuss their possible significance.

From these figures a number of conclusions may be drawn, but it is desirable to make several observations about certain of them. Firstly, it would appear that scoline prolongs twitching time (Phase 1), but there was one figure of 115 seconds in the scoline series which lies nearly five standard deviations outside

TABLE I
Mean Recovery Times

			No. of Treatments in Series
Phase 1:			
Straight E.C.T.	.. 40 seconds	$\sigma = 5.9$	55
E.C.T. with scoline alone	.. 46 seconds	$\sigma = 14.25$	54
Pentothal plus scoline	.. 34 seconds		57
Transithal plus scoline	.. 39 seconds		57
Phase 2:			
Straight E.C.T.	.. 40 seconds	$= 6$	
E.C.T. with scoline alone	.. 100 seconds	$\sigma = 36$	
Pentothal plus scoline	.. 72 seconds		
Transithal plus scoline	.. 54 seconds	$\sigma = 17.5$	
Phase 3:			
Straight E.C.T.	.. 5.8 minutes	$= 2.12$	
E.C.T. with scoline alone	.. 7.3 minutes	$\sigma = 2.73$	
Pentothal plus scoline	.. 10.4 minutes	$\sigma = 3.43$	
Transithal plus scoline	.. 7.6 minutes	$\sigma = 3.07$	

the mean, and this may be an artefact. Consequently significance was also calculated when this figure was left out. Secondly, as the stimulus was, on the average, given thirteen seconds earlier when scoline alone was used, it might be argued that the apparently greatly prolonged Phase 2 was simply due to the fact that the scoline had been given relatively later. Consequently a correction was made for this by deducting thirteen seconds from all Phase 2 times in the scoline series, so that the scoline effect could be approximately standardized. As will be seen, however, the effect of scoline on Phase 2 was so marked that the correction does little to influence the final results.

Certain conclusions may be drawn from the above findings, and some of them will then be discussed. It will be better, however, to mention them by the various phases to which they apply.

Phase 1

If we compare the duration of this phase, when scoline is used, with its duration in "straight" E.C.T. we find, as mentioned above, that the difference is highly significant ($P = .1$ per cent.). Possibly, however, the figure of 115 seconds twitching in one of the scoline cases was due to an unusual response to the scoline rather than to the fit itself. If this were so then this figure would have to be left out of the calculations, and if this is done the difference becomes "probably significant"; (Table II (a) and (b)) so there remains a distinct possibility that the addition of transithal or pentothal to scoline reduces the twitching time. However, our present figures do not warrant any firm conclusion.

Phase 2

Scoline very significantly delays the return of breathing (Table II (d)), but the use of transithal in addition to it strongly antagonizes this effect of scoline (c), as apparently does pentothal also, to a lesser extent.

Phase 3

Scoline very significantly delays recovery of consciousness, but pentothal has a very marked effect over and above that due to scoline. (It may be, as has been suggested, that the breakdown products of scoline have a depressant effect on consciousness.) It is very highly probable, however, that transithal does not

significantly delay recovery time beyond the inevitable delay caused by the scoline which is given with it. (Table II (e), (f), (g), (h), (i).)

Some of our findings are, of course, of purely academic interest, and lie rather in the field of pharmacology; others, on the other hand, are of great practical value. Before dealing with these, however, it is worth answering a few further objections which may be made at this stage about the technique and findings. Firstly, there was no significant difference in the equivalent doses of pentothal and transithal used. With both drugs sleeping and not anaesthetic dosages were given. Secondly, it might be thought that larger doses of scoline would have been used in the absence of an anaesthetic, but this was not so. If anything, the tendency was to use smaller doses. Again, it might be argued that the prolonged apnoea after scoline was due to excessive insufflation of oxygen, but, in fact, the minimum was used in order better to judge when breathing restarted.

We confirm that the use of scoline without anaesthetic has certain advantages (1, 2) and that it gives a satisfactory modification of the fit. Although we watched for them, we had no complaints of any of the unpleasant side effects described by Meyerhofer (4). We did, however, note that scoline prolonged Phase 2.

We found, in addition, that the use of transithal with scoline had many advantages over the use of scoline alone. Respiration became re-established much more quickly, so that insufflation was rarely necessary and the patient was anaesthetized, which obviated the danger of a "stun" shock followed by a return of consciousness before the painful scoline effect was over. We found that, if the injection was given reasonably slowly, there were no side effects beyond occasional coughing of a mild degree, and the drug did not delay the return of consciousness. The only disadvantages which our technique has over that using scoline alone is that the danger of intra-arterial injection remains, and a two-syringe technique is still required. Neither of these failings appears of great enough importance, if due care is exercised, to outweigh the advantages of the technique.

If, however, we compare transithal with pentothal as an anaesthetic for E.C.T., there seems little doubt that transithal is greatly superior, as the patients breathe much sooner and also regain consciousness very much more quickly. Not only does this facilitate their early co-operation in removal to a recovery room; it also saves nurses' time. Although it has not been measured objectively, it has frequently been remarked by the nursing staff that the patients are ready to go home far sooner, and it is certainly a fact that clinics finish far earlier than they used to do when pentothal was used (5).

CONCLUSION

Transithal possesses nearly all the advantages of the technique where scoline alone is used, the drug is greatly superior to pentothal as an anaesthetic for E.C.T., and there appear to be few if any dangerous side effects. Its use results in a pronounced saving in the time of both staff and patients.

TABLE II
"t" Test Results

Phase 1:

(a) Scoline	Mean time 46 seconds
(b) Straight	Mean time 40 seconds

$$t=3.94 < 1 \text{ per cent. level}$$

If figure of 115 excluded, $t=2.09$ (5 per cent. level)

Phase 2:

- (c) Scoline alone compared to scoline plus transithal—
 $t=8.45$ uncorrected. $t=6.05$ (corrected) <0.1 per cent. level, "Highly significant"
- (d) "Straight" and scoline—
 $t=4 <0.1$ per cent. level, "Highly significant"

Phase 3:

- (e) "Straight" and scoline — $t=3.55 <0.1$ per cent. level, "Highly significant"
- (f) Scoline and pentothal
 with scoline — $t=5.3 <0.1$ per cent. level, "Highly significant"
- (g) Pentothal plus scoline and
 transithal plus scoline — $t=4.5 <0.1$ per cent. level, "Highly significant"
- (h) Scoline and transithal
 plus scoline — $t=0.54$. No significant difference

SUMMARY

Comparison was made of the recovery times from E.C.T. using four methods:

- (1) "Straight" E.C.T.
- (2) E.C.T. with scoline but no anaesthetic.
- (3) E.C.T. with scoline and thiopentone sodium.
- (4) E.C.T. with scoline and buthalitone sodium.

It was found that scoline greatly delayed the return of breathing, a delay which was antagonized by buthalitone. The latter drug is judged to be superior as an anaesthetic for E.C.T. because the patient breathes much sooner and regains consciousness earlier, although safety is in no way sacrificed. The time of patients and staff is therefore greatly spared.

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STUDY OF CALCIUM METABOLISM IN ELECTRIC CONVULSIVE THERAPY (E.C.T.) IN CERTAIN MENTAL DISEASES

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For this study, calcium level in the blood was studied in 20 normal individuals between the ages of 20 and 25 years to find out the normal serum-calcium level in them in order that this finding might serve as a control. It was found to vary from 8·9 to 11·0 mg. per cent. (see Table I).

TABLE I
Serum-Calcium Levels in Normal Healthy Individuals

Sl. No.	Name	Serum Calcium in mg./100 c.c.
1	A.K.S.	9·7
2	B.S.M.	9·1
3	R.L.B.	9·3
4	K.P.M.	9·8
5	K.P.G.	10·2
6	H.S.L.	9·6
7	H.L.K.	8·9
8	P.C.A.	10·5
9	I.M.S.	10·0
10	S.R.A.	10·9
11	M.H.	9·5
12	Z.I.	9·8
13	S.C.S.	9·4
14	R.V.	11·0
15	S.R.	9·8
16	C.M.K.	9·4
17	P.L.T.	9·1
18	R.L.	9·0
19	C.L.	8·9
20	C.K.	9·0

The study of calcium metabolism in electric convulsive therapy was carried out in 15 patients. These cases had been admitted to the Mental Hospital, Agra, where E.C.T. was given for their treatment. These cases belonged to three different diseases—Schizophrenia, Manic Depressive Psychosis and Melancholia. All these cases were males and they had no other disease excepting their mental disorder. During the period of the E.C.T. they were given normal vegetarian diet. No calcium was administered by mouth or by parenteral route.

METHOD

Six to seven c.c. of blood was drawn from a vein in the ante-cubital fossa by a dry, sterilized syringe and the separated serum analysed. Samples of blood were collected immediately before, immediately after E.C.T., then 2 hours after E.C.T., 4 hours after E.C.T., 10 hours after E.C.T., then 24 hours after the first E.C.T., and finally 2 days i.e. 48 hours after the 10th E.C.T. These timings were chosen arbitrarily to study if any change was brought about in the calcium level by the E.C.T., how long these changes persisted and if there was any cumulative effect of these shocks on blood calcium. For the latter purpose calcium level was estimated after 48 hours of the 10th shock as it was thought that a result after less than the 10th shock might not give a true index of the cumulative effect on blood calcium.

It might be mentioned that E.C.T. was given on alternate days in almost all the cases studied. So the 10th shock was given roughly on the 22nd day of initial E.C.T. No treatment being given on the intervening Sundays.

The technique employed for the estimation of serum calcium was that of Kramer and Tisdall.

Seven cases belonged to the Schizophrenic group, six cases to Manic Depressive Psychosis and two cases to the Melancholic group. (See Table II.)

TABLE II

Diagnosis	No. of Cases
Schizophrenia	7
Manic depressive psychosis	6
Melancholia	2

Results of calcium estimation at different times in different cases (Table III).

The maximum, minimum and average values of serum-calcium level in 7 cases of Schizophrenia, 6 cases of Manic Depressive Psychosis, and 2 cases of Melancholia were as shown in Table IV, Table V and Table VI.

The maximum, minimum and average values in normal cases and mental patients are shown in Table VII.

OBSERVATIONS

Schizophrenic cases (see Graphs 1, 2, 3, 4, 5, 6 and 7).

Immediately after E.C.T. there was a rise in the serum-calcium level in all the cases. The deviation from the initial level varied from $+0.6$ to $+1.6$ mg. per cent.

Two hours after the E.C.T. there was a decline in the serum-calcium level in 5 cases (71.43 per cent.). The deviation was to -0.6 mg. per cent. In 2 cases (28.6 per cent.), however, a rise was noticed to $+0.6$ mg. per cent. above the initial level.

In all these cases, 4 hours after E.C.T. serum-calcium level was below the initial level. The fall varying up to -1.4 mg. per cent.

The fall was maximum at 10 hours after E.C.T. in all the cases except 3, in one of which the level had slightly risen from the level obtained at 4 hours after shock and in the other two, the lower level was maintained at the same figures as was obtained at 4 hours after E.C.T.

The fluctuation in the serum-calcium level that occurred between the highest rise and the lowest fall, varied between 1.2 to 2.4 mg. per cent., giving an average of 1.9 mg. per cent.

TABLE III

Serial Numbers of the Case

Time of Collecting Specimen

Serum Calcium in mg./100 c.c.

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Immediately before shock	..	10.4	10.8	9.2	9.5	10.0	9.2	9.0	8.8	10.4	8.6	9.4	10.0	9.3	8.8
Immediately after shock	..	..	11.4	11.1	10.9	11.2	10.8	11.3	10.1	11.0	9.8	11.2	10.8	11.5	9.4
After 2 hours	..	..	9.8	10.4	10.6	9.1	9.8	10.6	9.6	10.2	8.4	10.4	10.2	10.7	8.9
After 4 hours	..	..	9.0	10.2	10.0	8.8	9.6	8.8	9.2	9.6	8.2	10.0	9.6	9.8	8.6
After 10 hours	..	..	9.2	10.2	9.6	8.6	9.4	8.6	8.4	9.2	8.0	9.2	9.6	9.0	8.6
After 24 hours	..	..	10.4	10.8	9.2	9.4	10.0	9.4	9.0	8.8	10.2	8.6	10.0	9.2	8.8
After 10 shocks	..	..	10.4	10.8	9.2	9.6	10.2	9.4	9.0	9.0	10.4	8.6	10.0	9.4	9.0

TABLE IV

Schizophrenia			Immedi- ately Before Shock	Immedi- ately After Shock	After 2 Hours	After 4 Hours	After 10 Hours	After 24 Hours	After 10th Shock
			Values in mg./100 c.c.						
Maximum	..	..	10.8	11.6	10.2	10.2	10.2	10.8	10.8
Minimum	..	..	9.2	10.8	9.1	8.8	8.6	9.4	9.4
Average	..	..	10.02	11.1	9.9	9.4	9.2	10.0	10.1

TABLE V

Manic Depressive Psychosis			Immedi- ately Before Shock	Immedi- ately After Shock	After 2 Hours	After 4 Hours	After 10 Hours	After 24 Hours	After 10 Shocks
			Values in mg./100 c.c.						
Maximum	..	..	9.8	11.5	10.7	10.2	9.6	9.6	9.8
Minimum	..	..	8.6	9.4	8.4	8.2	8.0	8.6	8.6
Average	..	..	9.0	9.8	9.8	9.3	8.8	9.0	9.1

TABLE VI

Melancholia			Immedi- ately Before Shock	Immedi- ately After Shock	After 2 Hours	After 4 Hours	After 10 Hours	After 24 Hours	After 10 Shocks
			Values in mg./100 c.c.						
Maximum	..	..	9.4	11.3	10.6	10.0	9.2	9.4	9.4
Minimum	..	..	9.0	11.2	10.4	8.8	8.4	9.0	9.0
Average	..	..	9.2	11.25	10.5	9.4	8.8	9.2	9.2

TABLE VII

A Comparative Chart Showing the Maximum, Minimum and Average Serum-Calcium Levels in Normal Individuals and Mental Patients

Serum-Calcium Level				Normal Cases	Mental Patients
Maximum	..	..	..	11.0 mg./100 c.c.	10.8 mg./100 c.c.
Minimum	..	..	..	8.9 mg./100 c.c.	8.6 mg./100 c.c.
Average	..	..	..	9.5 mg./100 c.c.	9.4 mg./100 c.c.

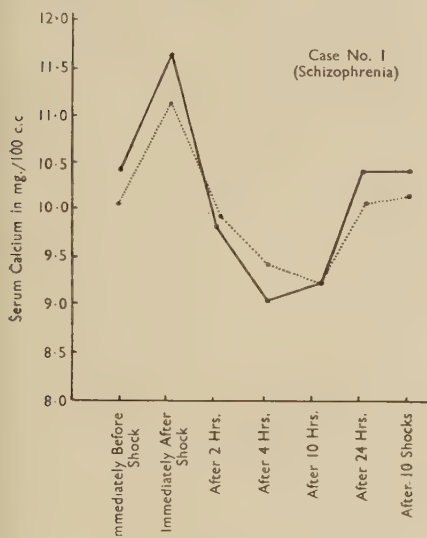
In all these cases, after 10 hours of the E.C.T. a rise was noticed in the serum-calcium level till 24 hours after the E.C.T. At 24 hours, the values returned practically to the initial level. In only one case, the level was slightly higher than the initial, and in two cases it was slightly lower, the deviations varying from $+0.2$ to -0.1 mg. per cent.

After 10 shocks, i.e. after 22 days of the initial E.C.T. the deviation from the initial level varied only but little from $+0.1$ to $+0.2$ mg. per cent.

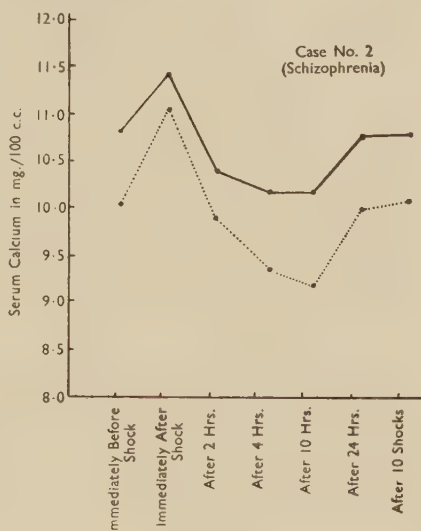
Manic Depressive Psychosis group (see Graphs 8, 9, 10, 11, 12 and 13).

Immediately after the initial E.C.T. there was a rise in the serum calcium in all the cases of this group. The deviation from the initial level varied from $+0.6$ to $+0.3$ mg. per cent.

Two hours after the E.C.T., there was a decline in the serum-calcium level but the serum-calcium level at this stage was higher than the initial in 5 cases (83.3 per cent.) and low in one case (16.6 per cent.). The deviation from the initial level varied from -0.2 to $+1.4$ mg. per cent.

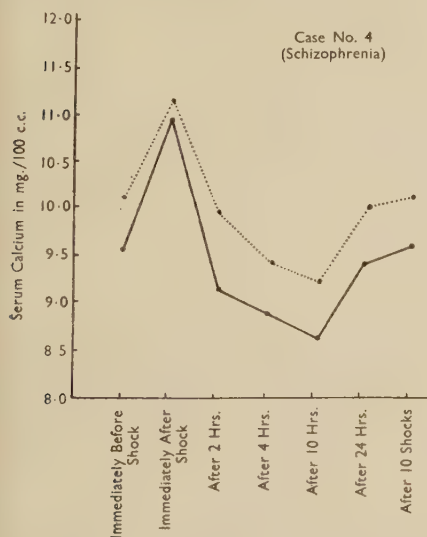


GRAPH 1

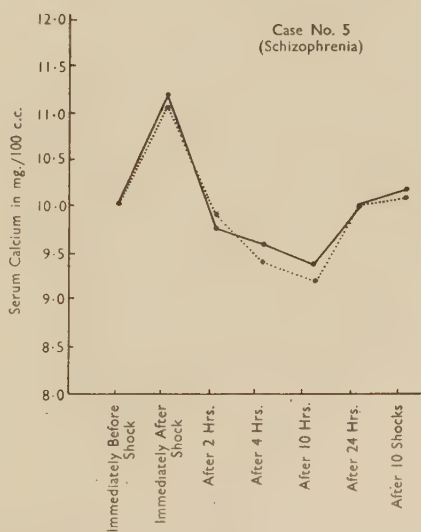


GRAPH 2

At 4 hours after the E.C.T. the serum-calcium level was lower than that observed at 2 hours, but it was higher than the initial level in 4 cases and lower than the initial level in 2 cases. The deviation from the initial level ranged from -0.2 to $+0.8$ mg. per cent.



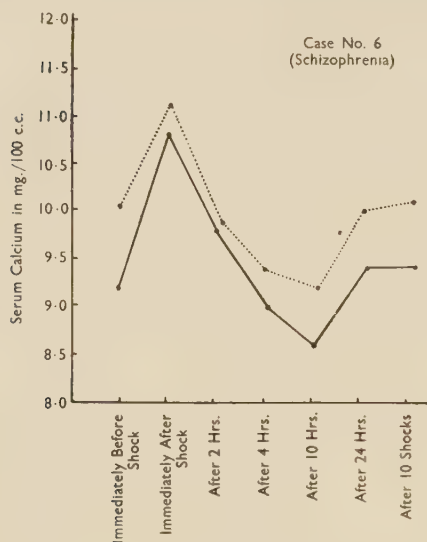
GRAPH 3



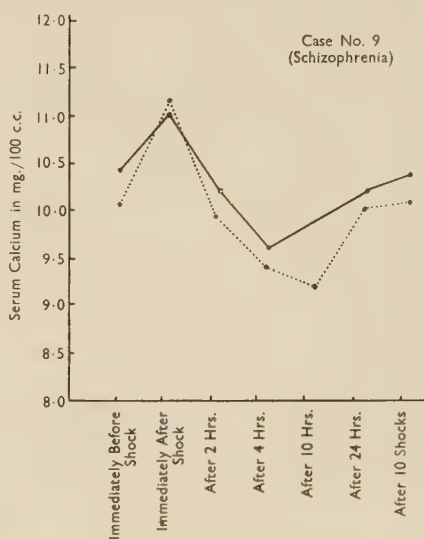
GRAPH 4

At 10 hours after the E.C.T. the fall in the serum-calcium level was noticed in 5 cases and in one case where the maximum fall had been observed at 4 hours after E.C.T. that level was still maintained up to this time. The serum-

calcium level was observed to be lower than the initial level in 5 cases excepting in one case where the level was higher than the initial. The deviation from the initial ranged from $+0.4$ to -0.6 mg. per cent.

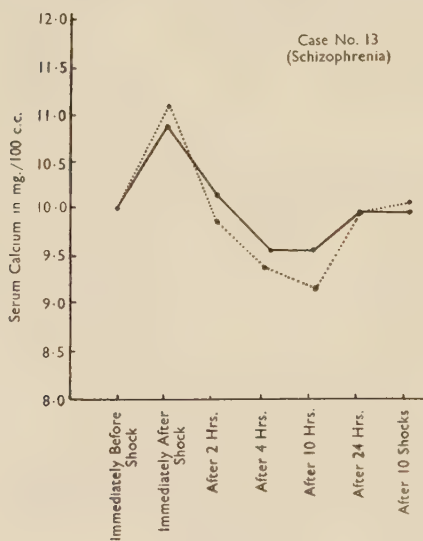


GRAPH 5

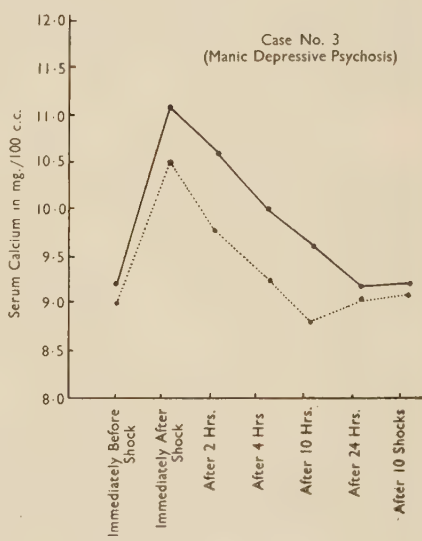


GRAPH 6

The fluctuation in the serum-calcium level that occurred between the highest rise and the lowest fall varied between 1.5 to 2.5 mg. per cent. giving an average of 1.7 mg. per cent.



GRAPH 7

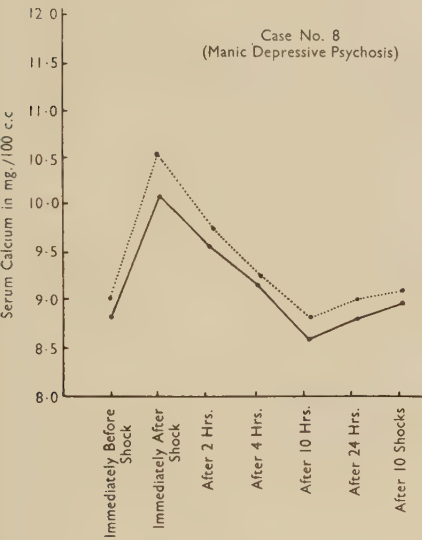


GRAPH 8

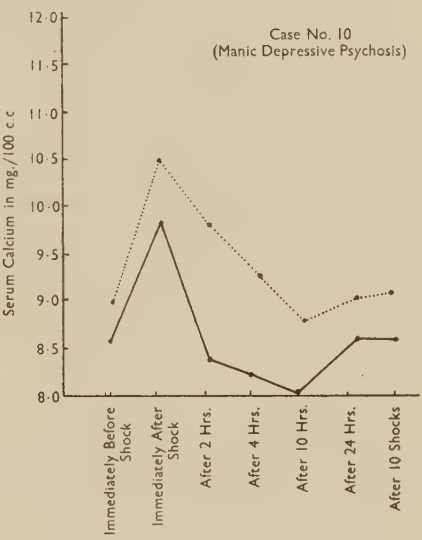
After 24 hours, it was noticed that there was a rise in the serum-calcium level from that observed at 10 hours after E.C.T. except in one case where there was a further fall. As compared with the initial level, there was a slight decrease in the serum-calcium level in 2 cases while in others it was at the same level as the initial one. The deviation was up to -0.2 mg. per cent.

After 10 shocks, the deviation from the initial level varied from +0.1 to +0.2 mg. per cent.

Melancholic group (see Graphs 14 and 15).

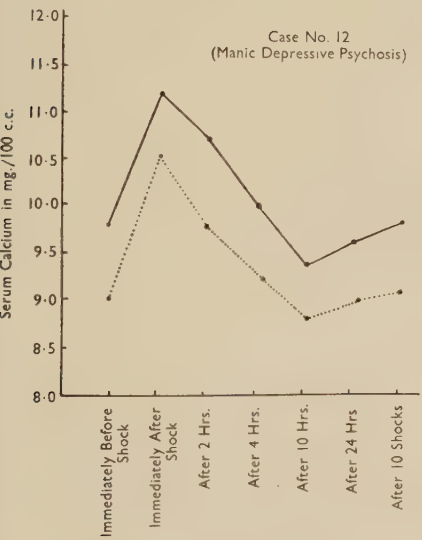


GRAPH 9

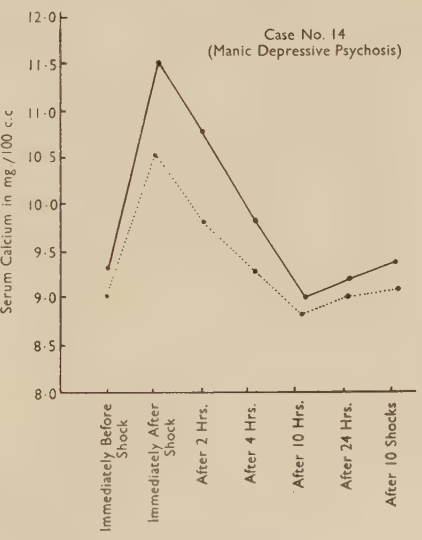


GRAPH 10

Immediately after E.C.T. there was a rise in the serum-calcium level in all the cases. The deviation from the initial level varied from +1.8 to +2.3 mg. per cent.



GRAPH 11

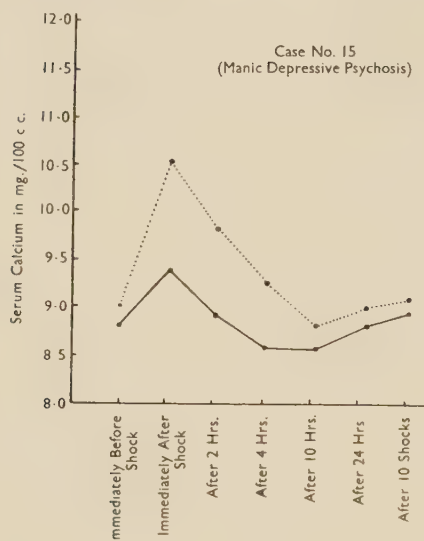


GRAPH 12

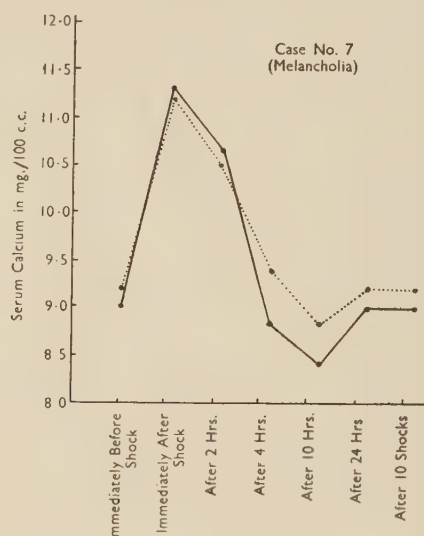
Two hours after the shock, there was a decline but the serum-calcium level was higher than the initial. The deviation from the initial level noticed was from +1.0 to +1.6 mg. per cent.

After 4 hours of E.C.T. although the serum-calcium level was lower than that observed at 2 hours, it was higher than the initial level in one case and lower

in one case. The deviation from the initial level ranged from -0.2 to $+0.6$ mg. per cent.



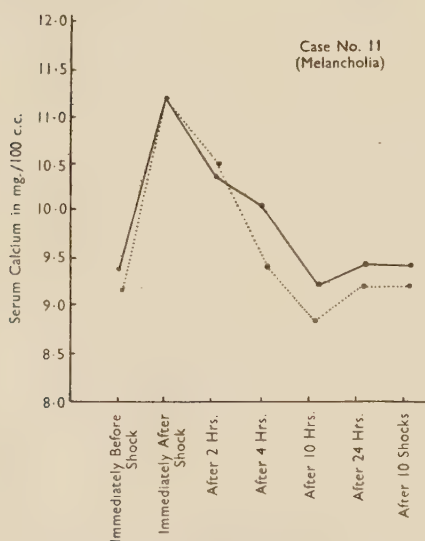
GRAPH 13



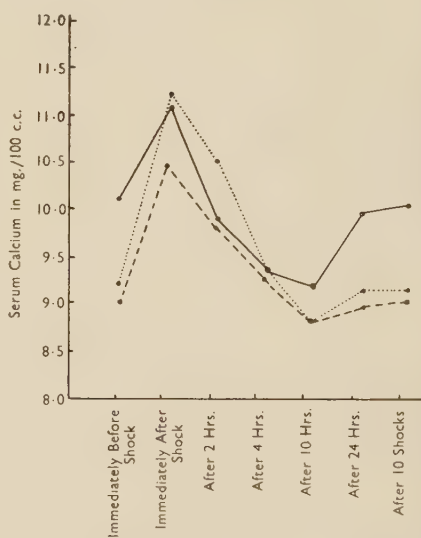
GRAPH 14

The fall was maximum at 10 hours in all the cases. The values at this time were -0.2 to -0.6 mg. per cent. below the initial level.

The fluctuation in the serum-calcium level that occurred between the highest rise and the lowest fall varied from 2.0 to 2.9 mg. per cent.



GRAPH 15



GRAPH 16

- Average cover in Melancholia
- Average cover in Schizophrenia
- - - - Average cover in Manic Depressive Psychosis

After 10 hours there was a rise in the serum-calcium level till 24 hours when it reached the initial level.

After 10 E.C.T. the serum-calcium level was the same as the initial one.

RESULTS

From a comparison of the changes observed in the serum-calcium level during E.C.T. in the three groups of cases (see Graph 16) we find that:

1. The average initial level of calcium in the Melancholic and Manic Depressive Psychosis groups was almost the same, but that in the Schizophrenic group was slightly higher although all of them were within normal limits.

2. The average rise in serum-calcium level that occurred immediately after the shock was maximum in the Melancholic, then in the Manic Depressive Psychosis, and least in Schizophrenic group. This more or less correlated with the improvement that the patients showed as a result of E.C.T. For example, in Cases Nos. 3, 7, 11 and 24 where the rise averaged 2.05 mg. per cent., the patients had shown very marked improvement as compared to Cases Nos. 2, 4, 6, 9, 10, 13 and 15 where the average rise had been 1.1 mg. per cent. and who showed little or no improvement.

3. The maximum fall, which occurred 10 hours after shock from the peak rise was greatest in the Melancholic group, next in the Manic Depressive Psychosis group and least in the Schizophrenic group. This also more or less correlated with the improvement as observed clinically. For example, in Cases Nos. 7, 11, 12 and 14 who showed the greatest improvement, the average fall was by 2.3 mg. per cent., whereas in Cases Nos. 9, 10, 13 and 15 where the average fall was by 1.4 mg. per cent., the improvement was negligible.

DISCUSSION

From the study of 30 normal healthy individuals and the values obtained in the mental patients before shock therapy, it was observed that the serum-calcium level in the latter differed in no way from those of the former (see Table VII).

The various mental disorders from which the patients were suffering were Schizophrenia, Manic Depressive Psychosis and Melancholia. There was no difference observed in the serum-calcium levels of patients of these disorders. Probably the mental disorders do not produce any significant changes in the blood-calcium level or they are so minute that with the common techniques employed they are not detectable.

A study of calcium metabolism during E.C.T. reveals that E.C.T. leads to an immediate hypercalcaemia but after 2-4 hours, there is a decline, the maximum decrease being after 10 hours of the E.C.T. By the next day, the levels reach practically the initial levels.

In a previous study on "Biochemical and Haematological changes during E.C.T." (2) done in this department, it was observed that E.C.T. probably acts as a stress agent which sets forth the changes which Selye described as "Stress response" of the General Adaptation Syndrome. Thus E.C.T. stimulates the anterior pituitary which secretes A.C.T.H. and this in turn acts on the bone-marrow and calcium is mobilized from there. This leads to an increase in the serum-calcium levels as observed in our studies also. As the levels of serum calcium regain initial levels within 24 hours, it is believed that E.C.T. acts as a brief stress only.

One very interesting finding in our study was that although the pattern of response to E.C.T. was the same in all the cases, the degree and the extent

of fluctuations in the serum-calcium level varied in different cases. Thus, the average rise in serum-calcium level that occurred immediately after the shock was maximum in the Melancholic group, then in Manic Depressive Psychosis group, and least in the Schizophrenic group. This more or less correlated with the clinical improvement noticed as a result of E.C.T. For example, in Cases Nos. 3, 7, 11 and 14 where the rise in serum-calcium level averaged 2.05 mg. per cent., the patients had shown very marked improvement as compared to Cases Nos. 2, 4, 6, 9, 10, 13 and 15 where the average rise had been by 1.1 mg. per cent., and who showed little or no improvement.

Again, the maximum fall which occurred at 10 hours after E.C.T. from the peak rise (observed immediately after shock) was greatest in the Melancholic group, next in Manic Depressive Psychosis group and least in Schizophrenic group. This is also more or less correlated with the clinical improvement. For example, in Cases Nos. 7, 11, 12 and 14 who showed the greatest improvement, the average fall was by 2.3 mg. per cent., whereas in Cases Nos. 9, 10, 13 and 15 where the average fall was by 1.4 mg. per cent. the improvement was negligible.

This leads us to a very important conclusion which may be of immense value in assessing whether a particular patient who is undergoing E.C.T. is likely to improve with it or not. Thus, if we study the range of fluctuations in the serum-calcium level during the first 2 or 3 shocks and compare it with a "minimal standard fluctuation curve", we might be able to assess whether this particular patient is going to improve with E.C.T. or not, and thus save him from unnecessarily risking the hazards of this drastic procedure.

CONCLUSIONS

Immediately after E.C.T. a transient hypercalcaemia occurs. This rise in serum-calcium level is followed by a gradual fall and values even less than the initial level occur at 10 hours after the E.C.T. Multiple shocks do not lead to any significant change in the serum-calcium level. This pattern of response to E.C.T. is seen in all the cases but varies in the degree of fluctuation in different cases. It was observed that the degree of fluctuations was more in cases who showed marked clinical improvement and less in those who showed little or no improvement.

It has been tentatively suggested that by studying the pattern of response to E.C.T. during the first 2 or 3 shocks and comparing it with a "Minimal Standard Fluctuation Curve", it may be possible to assess whether that particular patient is likely to improve with E.C.T. or not and thus be saved from the hazards of such a drastic procedure.

SUMMARY

The study was undertaken with a view to study the changes in the calcium metabolism during E.C.T. in certain mental diseases.

No literature was available on the subject. The material selected and the technique employed for calcium estimation has been mentioned.

A study was undertaken in 20 normal cases to find the average normal values for serum calcium. Fifteen cases suffering from various mental disorders, who were given E.C.T. at the Mental Hospital, Agra, were selected for this study. Serum-calcium levels were estimated immediately before and immediately after E.C.T., 2 hours after, 4 hours after, 10 hours after and 24 hours after the initial E.C.T. and finally 2 days after the 10th E.C.T.

It was observed that E.C.T. leads immediately to hypercalcaemia, followed by a relative hypocalcaemia after 2-4 hours and a return to the initial levels 24 hours after the shock.

The possibility of avoiding the risks of the drastic procedure like E.C.T. in a patient, by studying the degree of fluctuation in the serum-calcium levels during the first 2 or 3 shocks and comparing it with a minimal standard fluctuation curve, has been suggested.

ACKNOWLEDGMENTS

We are highly indebted to Dr. R. S. Lal, Superintendent, Mental Hospital, Agra, for permitting us to carry on this work on his patients. We are thankful to Dr. M. G. Chakravarti, Reader in Biochemistry, for permission to do the calcium estimations in his laboratory. And last, but not the least, we feel obliged to Dr. H. N. Bhatt, Principal and Superintendent, S.N. Medical College and Hospital, Agra, for permitting us to publish this work.

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Reviews

Clinical versus Statistical Prediction. By PROFESSOR PAUL E. MEEHL. University of Minnesota Press, Minneapolis, 1954. Pp. 149. Price 24s.

The relative value of clinical methods as against those of psychometrics has been a disputed question ever since Binet and Simon suggested that simple standardized tests might advantageously replace the experienced clinician's general impression. Should patients be diagnosed from the results of a battery of tests, or after an interview? Or should both methods be used? And since the application of both would require twice the man-hours, could the available man-hours be better employed if more time were given to one and less to the other? And if so, to which? Professor Meehl has attempted a survey of the whole field to see if some order, some agreement, can be perceived where often there is visible only the dust of battle. He is well qualified to undertake the survey, for he is a practising psychiatrist who has had a thorough earlier training in the methods of rat psychology, of factor analysis, and of statistics.

It is sufficient for me to say that the book is excellent in its clarity, its fairness to all points of view, and its depth. The subject is undoubtedly a difficult one, for it demands both a mastery of theoretical methods in all their rigor and subtlety, and also the sound commonsense that marks the realist. Meehl shows abundantly the breadth and acuteness of his understanding; in questions of probability, for instance (which are bound to arise in this matter), he handles the somewhat tricky arguments surely and without confusion. Though treating a difficult subject, he writes clearly. Mathematical methods are not used in the text, though an understanding of their nature is expected of the reader. Briefly, the book can be strongly recommended to those who would like to come to a well-informed and well-balanced opinion on the matter.

But, as Meehl emphasizes, opinions are worth little; what matters is: how well do they work in practice? Perhaps his book will bring nearer the day when the two methods will be set to work side by side, in a series of fully controlled tests, so that we may leave the polemics and follow the method that does actually give our patients the best chance of recovery.

W. ROSS ASHBY.

Risk and Gambling. By JOHN COHEN and MARK HANSEL. Longmans, Green & Co., London, 1956. Pp. 153. Price 14s.

This little book has the subtitle "The study of subjective probability", which may mislead. The book studies its subject objectively, for it is largely concerned with experimental results obtained by the authors from tests with children; but it studies probability not in the sophisticated form of modern mathematics, dealing with frequencies, but in the form of how risk, uncertainty, and evidence interact in the mind of the child and eventually lead to action. Their main object is thus related to the work of Piaget: to discover how the developing mind actually does progress from the crudest and most primitive ideas up to sophistication—the information always being provided by the child's actual behaviour. The results are of considerable interest, and show clearly that the ordinary person operates with probabilities and risks in a way that by no means conforms to the elegant theorems proved by the mathematicians to minimize risk or to maximize gain. Not even internal consistency is shown, for a subject may assess a situation verbally in one way, and then show by his actions that he has assessed the risk otherwise.

The book covers many topics, from betting on beads in an urn to crossing the road through traffic. The topics are touched on lightly and the book contains a good deal of matter of minor interest. It shows, however, that the subject has many interesting facets, and deserves further study.

W. ROSS ASHBY.

The Sexual, Marital and Family Relationships of the English Woman. By DR. EUSTACE CHESSER, with collaborators. Hutchinson's Medical Publications, London, 1956. Pp. 642; 479 tables, 81 charts. Price 75s.

The aims of this study are, in the author's words:

- “(a) To discover from a sample of English women their present experiences in, and attitudes towards, marriage and sex relationships;
- “(b) To trace the associations between certain experiences of their childhood and their present experiences in, and attitudes towards, marriage and sex relationships;
- “(c) To discover those factors associated with their feelings of happiness and unhappiness in marriage.”

The method used was primarily by questionnaire, the forms being introduced to patients by 1,498 doctors who agreed to co-operate. The number of women who responded was 6,251.

Most of the book is taken up with largely factual analyses of the material obtained, though there is sufficient discussion of each table to give the reader some guidance. Guidance is certainly necessary, for many of the tables require much qualification if their interpretation is to be correct.

Its chief weakness is its difficulty in defining the group to whom the facts refer. The book's title says “. . . of the English woman”, but this suggestion must not be taken as final. Of the doctors approached, only about one-third acted, and of about 18,000 forms distributed, again about one-third were used. The group studied is therefore somewhat selected, for it will clearly contain few of the anti-social or indifferent, and an excess of the socially more conscious and scrupulous. This is the same fault that was found by the Committee of the American Statistical Association in their evaluation of the Kinsey Report. It is one of the major difficulties that any investigation of this type must encounter. Facts may be collected, but the difficulty remains: to what group do these facts refer? In Chapter 2 the author discusses these difficulties, and others, with good sense and frankness. He shows, in Tables 3–8, that the group actually studied is not accurately representative of the women of England, and he discusses how the data in the body should be interpreted with this in mind.

The investigation, then, is by no means perfect; but at the present time a statistically perfect method of sampling English womanhood does not exist. We can, however, ask whether the author has achieved the best that is practically achievable. In the reviewer's opinion he has. The matter is treated skilfully, and with good judgment. Those who use the book must remember, however, that the facts given require careful interpretation, and that many somewhat complex qualifications must be borne in mind if the results are not to be misleading.

W. ROSS ASHBY.

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Behavioral Changes of Chronic Schizophrenics in Response to Reserpine

The double-blind design was successfully employed in the evaluation of the effect of reserpine on the behavior of lengthily hospitalized chronic schizophrenics.

Reserpine in a dose of 3 mg. appears to have a salutary effect on the behavior of some of this population when evaluated by the testing devices described.

Seeming improvement was noted primarily in the areas of Accessibility and Contact Span.

Major toxicity did not occur.

(Authors' Abstr.)

Effects of Mescaline, LSD-25, and Adrenochrome on Depth Electrograms in Man

During depth-electrographic studies, mescaline and d-lysergic acid diethylamide (LSD-25) have been administered to five patients; two of them had psychosis with convulsive disorders, and three had chronic schizophrenia. The most striking observation in the epileptics was that mescaline and (LSD-25) had a pronounced quieting effect on the spike and sharp-wave foci in depth recordings. An increase in paroxysmal activity characterized by waves of 2 to 5 c.p.s. occurred in the patients who had chronic schizophrenia. Mescaline and (LSD-25) elicited the appearance of a double-spike focus deep in the temporal lobe in one epileptic patient.

The clinical effects and those on the depth electrogram produced by mescaline and (LSD-25) were reversed by the injection of chlorpromazine.

Intravenous administration of adrenochrome to two patients who had chronic schizophrenia produced an increase in paroxysmal activity manifested by waves of 2 to 5 c.p.s. An increase in paroxysmal activity of 2 to 3 c.p.s. occurred in a patient who had a psychosis with a convulsive disorder.

(Authors' Abstr.)

Model Psychoses Induced by LSD-25 in Normals. I. Psychophysiological Investigations, with Special Reference to the Mechanism of the Paranoid Reaction

d-Lysergic acid diethylamide (LSD-25) given to 25 normal adults produced widespread temporary alteration in psychic functioning which resembles most the undeteriorated schizophrenic reaction type.

Insight and detachment are sufficiently preserved to allow recognition of the split between thinking and feeling as reality control becomes disturbed.

Rorschach tests completed in 13 cases allowed a fairly good prediction of a psychotic-like change from the control to the drug test, though the type of model psychosis could not be reliably anticipated.

Paranoid-like reaction (14 cases) showed a better correlation with the occurrence of complex synesthesiae, especially when the cross influence involved the examiner's voice and thoughts, on the one hand, and the subject's thoughts, and bodily sensations, on the other.

Theories aiming at elucidating the working mechanism of this drug point toward enzyme inhibition, on the biochemical side, and predominant vulnerability of the temporal lobe-diencephalic circuits, on the neurophysiological side.

(Authors' Abstr.)

Model Psychoses Induced by LSD-25 in Normals. II. Rorschach Test Findings

Of 25 Rorschach studies obtained in the LSD and in the lucid state, 13 were of sufficiently good quality to permit comparison.

The number of responses and the factor of "sexuality" were the most reliable indicators to predict a psychotic reaction.

The Rorschach test furnishes considerably less precision with which the type of psychotic reaction can be predicted.

In the paranoid reactions alone, the occurrence of complex synesthesiae appeared to have more of a predictive value than the Rorschach test results.

(Authors' Abstr.)

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Communist Interrogation and Indoctrination of "Enemies of the State". Analysis of Methods Used by the Communist State Police

The methods used in Communist countries for the interrogation and indoctrination of persons regarded as enemies of the state have their roots in secret police practices which go back for many years. These methods have been refined and systematized by much use and experience. Data about these procedures have been collected and analyzed. The general dynamic features which underlie them are understandable.

Those who live in Communist states recognize that at times the state police are almost unlimited in their power and their action may be swift and arbitrary. When residents of such communities become aware that they are suspected by the police, their feelings of impotence and uncertainty are greatly augmented. As they are increasingly avoided by their friends and associates, they feel isolated and rejected, and develop intense anxiety, often colored by feelings of guilt. Their sudden seizure under dramatic circumstances is additionally traumatizing. They usually enter upon their prison experience feeling fearful, vaguely guilty, helpless, and completely uncertain of their fate.

When the initial period of imprisonment is one of total isolation, such as that used by the KGB, the complete separation of the prisoner from the companionship and support of others, his utter loneliness, and his prolonged uncertainty have a further disorganizing effect upon him. Fatigue, sleep loss, pain, cold, hunger, and the like augment the injury induced by isolation. The cumulative effects of the entire experience may be almost intolerable. With the passage of time, the prisoner usually develops an intense need to be relieved of the pressures put upon him and to have some human companionship. He may have a very strong urge to talk to any human and be utterly dependent upon anyone who will help him or befriend him. At about this time he also becomes mentally dull and loses his capacity for discrimination. He becomes malleable and suggestible, and in some instances he may confabulate.

The interrogator exploits the prisoner's need for companionship. He uses items from the prisoner's biography derived from police files, from the prisoner's associates, and from hours of interrogation to arouse further guilt, conflict, and anxiety. He makes use of the dependence of the prisoner, which is strengthened by the intimate sharing of information about his life. He frustrates and further disorganizes the prisoner by rejecting his statements. He scolds, punishes, and threatens him when he does not co-operate, and approves and rewards him when he does. Then, by suggesting that the prisoner accept half-truths and plausible distortions of the truth, he makes it possible for the prisoner to rationalize and thus accept the interrogator's viewpoint as the only way out of an intolerable situation.

The methods of interrogation and indoctrination used in Communist China are in many respects similar to those of the Russian state police, from which they were in part derived; but in some respects they are quite different because of the special needs and traditions of the Chinese. In the Chinese prison, the individual interrogator is still important, and in occasional cases the management of the prisoner may quite closely duplicate that of the KGB. But in most instances the efforts of the interrogator are supplemented by the effects of the interaction between the prisoner and six or eight of his fellow prisoners with whom he is incarcerated in a crowded cell. Here the group replaces the interrogator as the focus of the prisoner's relationships. In this setting of complete lack of privacy, there is an unremitting routine of self-criticism sessions, group-discussion sessions, rote learning, constant repetition of Communist viewpoints, and the repeated rewriting and rejection of autobiographical essays. The group exploits the feeling of emotional nakedness and unworthiness which the self-criticism sessions engender, dwelling upon items obtained from the prisoner's life history during these sessions which arouse in him guilt, conflict, and anxiety. These feelings are greatly potentiated when the group rejects, isolates, and reviles him because of his "improper" attitudes and past behavior. The prisoner is thus placed in a situation in which he cannot avoid having his past life reviewed and questioned and cannot avoid hearing an exposition of the Communist position. Moreover, for a period, sometimes of years' duration, he has access to nothing but Communist-oriented history and Communist interpretation of current events. Like the KGB interrogator, the group rewards and approves the prisoner when he co-operates and behaves in accordance with their aims, and thus indicates to him that the only possible solution to an intolerable situation is the acceptance of the "proper" point of view.

Under pressures such as these, prisoners usually rationalize a change in attitude and hold it for an indefinite time. In general, this change in attitude is only as great as the prisoner feels it must be to enable him to relieve himself of the intolerable pressures under which he labors. In the KGB pre-trial interrogation, the achievement of a successful rationalization and a satisfactory protocol is usually accompanied by a profound feeling of relief, and an unspoken agreement with the interrogator that may even have overtones of warmth and friendliness. In the Chinese group cell, where the pressures are much more prolonged and the demands upon the prisoner are correspondingly more intense, the ultimate achievement of a proper rationalization and group acceptance is associated with feelings of relief that are occasionally exhilarating and sometimes show some of the features of a religious "conversion".

Men under the complete control of Communist police have been made to say and do many things which their captors desire. Some people have proved to be much more malleable than others; but even under the most strenuous circumstances some men are remarkably refractory and refuse to co-operate with their captors up to the point at which they develop confusional states and delirium. The most effective features of the Communist procedures are those which would operate even in the absence of control. Prisoners who were not excessively abused and who encountered men who appeared to be dedicated, selfless, and even "idealistic" in their attachment to the ostensible goals of Communism have acknowledged these features of their captors; and those who were presented with plausible evidence have accepted it tentatively. When they have discovered that they would be rejected, reviled, and punished for non-co-operative behavior, they have refrained from doing or saying anything which would bring such abuse upon them when they were in Communist control. Those whose past lives have been colored by feelings of much guilt, by lack of purpose or commitment, and those who were previously sympathetic to Communist views have been more amenable to the Communist methods.

Prisoners who have been released from Communist control and have been able to assure themselves that they will no longer be punished for "improper" opinions have gradually readjusted their attitudes to their new environment. Their memories of the punishments and brutalities which they have endured have been lively. For most prisoners these memories

override all others. When they have felt safe to acknowledge their resentment, they have expressed extreme hostility toward those responsible for their bad prison experiences, and they have nearly always rejected Communism and all those connected with it.

(Authors' Abstr.)

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A Study of Thalamo-Striate Relations in the Monkey

1. The retrograde cell degeneration in the thalamus following lesions in the corpus striatum has been studied in the monkey.
2. The centromedian and parafascicular nuclei have been shown to project to the putamen only; within the projection there is a precise topical organization.
3. The intralaminar nuclei, centralis medialis, paracentralis and centralis lateralis project to the head of the caudate nucleus in a similarly organized manner.
4. The nucleus reunions is directly related to the cortex of the infralimbic area (area 25); the parataenial nucleus probably projects to the nucleus accumbens and the paraventricular nuclei appear to be related to the lateral pre-optic area.
5. These findings have been discussed in relation to the anatomical literature and to the physiological concept of a diffuse thalamo-cortical projection system.

(Authors' Abstr.)

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Surface and Depth Electrography of the Frontal Lobes in Conscious Patients

In a group of 5 psychotic and 4 non-psychotic patients the electrical activity of the depth of the frontal lobes was recorded for 1 to 4 days, using multilead needle electrodes. Electrical

activity of the motor and pre-motor areas was also recorded in 4 of these patients by plate electrodes. Results were as follows:

1. Typical patterns of electrical activity repeatedly observed included: (a) low voltage; (b) alpha-like; (c) high voltage fast; (d) high voltage slow irregular; (e) paroxysmal rhythmic slow; (f) spikes and sharp waves; (g) other types. Some rhythms seemed to be located in determined regions of the frontal lobes.

2. Very similar spatial distribution of rhythms within the frontal lobes was found in patients with different types of illness.

3. In general the electrical activity was symmetrical in both sides of the frontal lobes. In one patient the activity was asymmetrical.

4. No correlation could be established between electrical activity of the depth of the frontal lobes, and motor movements, sensory stimulations, or changes in the mood of the patient.

5. Voluntary motor movements inhibited the electrical activity of specific cortical motor areas. Sound stimulation of the patient with music or noises increased the electrical activity of motor and pre-motor areas.

(Authors' Abstr.)

The Form, Voltage Distribution and Physiological Significance of the K-Complex

1. A study has been made of the morphological features of the K-complex during consciousness, sleep, and barbiturate anaesthesia, in 80 psychiatric patients and 15 normal subjects. No differences between the two groups of subjects came to light. During consciousness the phenomenon is elicitable in about 20 per cent. of subjects but is prominent only in about one-third of these.

2. To the "sharp wave" component, the only one clearly discernible in the awake subject, there are added in unconsciousness one or more high voltage slow waves followed by a burst of "spindles". Both the latency and the duration of the sharp wave component are increased during sleep and barbiturate anaesthesia. In these and other states of diminished awareness the phenomenon is universally present, it is abolished in deep unconsciousness.

3. Evidence is presented suggesting that the K-complex is specially adapted for responding to transient disturbances in the environment. Swift adaptation occurs to continual stimuli; those given at shorter than 2-3 seconds intervals in sleep are interpreted as continual. Different forms of stimulation evoke identical K-complexes.

4. Studies of the voltage distribution of the K-complex by a special technique reveal congruent distributions during both first and second components which are consistent with the activity of a stationary source or sources. The widespread distribution of these two phases is hence attributed to their being mediated by some diffuse projection system to the cerebral cortex.

5. Attention is drawn to the similarities between the K-complex with its initial electro-positive deflection and after-discharge, and the localized changes of smaller voltage evoked in specific sensory areas by various types of afferent stimulation. The K-complex is however distributed over a diffuse area and is identical for different stimuli. There is evidence to suggest that it is the correlate of a crude perceptual process which tends to initiate arousal. Such a theory satisfactorily accounts for the known facts about the complex; its non-specificity, diffuse distribution, and its presence in prominent form only in states of unconsciousness.

(Authors' Abstr.)

The Relation Between Electro-cortical Waves and Responsiveness of the Cortico-spinal System

1. Studies of the responsiveness to electrical stimulation of the system of cortical neurones giving rise to evoked volleys in the medullary pyramid have been carried out in cats.

2. Phasic variations in responsiveness and relayed activity have been correlated with phasic changes of cortical surface potential during the elicitation of augmenting waves and during spontaneous spindle waves. Relayed pyramidal volleys appear during the initial surface positivity of each of these cortical wave forms. During the positive and early negative phases of the surface potential waves, the responsiveness of the efferent neurone system is augmented. During the late phase of cortical negativity, and for 50-150 m./sec. thereafter, responsiveness of the efferent neurone system is depressed. These changes affect the indirect portion of the evoked pyramidal volley to a significantly greater extent than the direct portion of the volley.

3. Responsiveness of this corticofugal neurone system has also been tested during the elicitation of that particular form of recruitment wave which develops upon stimulation of the midline nuclei of the diffuse thalamic projection system. During this type of recruitment response there is no relayed pyramidal volley. Neither is there any detectable alteration in responsiveness of the efferent neurone system.

4. The marked differences in the effects associated with these electrically similar potential changes indicate the existence of at least two fundamentally different mechanisms subserving thalamo-cortical relationships.

(Authors' Abstr.)

The Electroencephalogram in Dementia. Some Preliminary Observations and Correlations

1. In 71 patients with dementia of various aetiologies, 55 had abnormal EEG records. Of these 52 showed non-focal slow waves, and 3 predominantly fast records.

2. In the overall group of patients, there was a statistically significant relationship ($p < .01$) between the degrees of dementia (operationally defined as progressive involvement of categories of the sensorial examination) and the degree of EEG slowing.
3. The number of demented patients with major disturbances of mood, thought content and behavior, and EEG slowing, was not greater than those who only had uncomplicated sensorial defects, nor could they be differentiated by EEG alone.
4. Regardless of etiology of the dementia, there would appear to have been a statistically significant correlation between clinical evidence of the severity of dementia and EEG slowing.
5. Sixteen of 71 patients had normal EEG records, none of whom had the severest degree of sensorial dislocation.
6. The possible significance of these findings is discussed in the light of previous work.

(Authors' Abstr.)

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Selection of Patients for Treatment with Frenquel

A preliminary study of a new anti-hallucinogenic agent, Frenquel, has been carried out. Female patients suffering from auditory hallucinations were selected from a single Ontario Hospital for the study. These numbered 64, of whom 56 had continuous hallucinations and 8 had intermittent hallucinations which were absent at the start of the trial. Twenty-five patients were given a placebo, and 39 received Frenquel. The dosage was one 20 mg. tablet three times a day by mouth for 14 days, and patients were observed until they had reverted to their initial condition. This took place in all cases within 10 days of cessation of treatment. No toxic effects were observed.

Sixty-four per cent. of patients receiving the placebo were unaffected, while only 28 per cent. of those given Frenquel showed no change. Six of the latter group of 11 were suffering from intermittent hallucinations which were absent at the start of the trial; one had a history of head injury as the initial incident in her illness; and two were among the only three patients in the entire group who had been in hospital for more than 30 years.

Twenty-three per cent. of the 39 who received Frenquel showed minimal or mild improvement in mood, sociability and interest. It was held that this result could not be fairly ascribed to the action of the drug.

Forty-nine per cent. of the Frenquel group showed complete or marked diminution of auditory hallucinations, in association with diminution or cessation of delusions and in-

appropriateness of mood together with improvement in mood, behavior, sociability and interest.

It was held that intermittent hallucinations contraindicated the use of Frenquel, and suggested that a stay in hospital of 30 years or more was an unfavorable factor.

Age, length of hospitalization up to 30 years, and the presence of a family history of mental illness were shown to have no significance in determining the selection of patients for treatment with Frenquel.

Patients with normal pre-psychotic personalities, with marked delusions of all types, in association with incongruity of affect, without intellectual deficits, but showing little antagonism are those most likely to benefit from the drug.

It is suggested that further and more intensive studies in this limited field are now called for, and that higher dosage over a longer period or a maintenance dose of this drug may well prove a more than useful weapon in the psychiatric armamentarium.

(Author's Abstr.)

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Diffuse Degeneration of the Cerebral White Matter in Severe Dementia Following Head Injury

It has been shown that a closed, apparently uncomplicated head injury may be followed by a diffuse degeneration of the white matter and consequently may completely and permanently incapacitate the patient. The extreme dementia produced by such a lesion should not be confused with coma, which has different physical signs. The nerve fibre degeneration is not due to cortical cell loss, nor to infarction or laceration of brain substance. Its pathogenesis has not been determined but evidence points to physical damage of nerve fibres at the time of injury as a likely cause.

It is at present not possible to say how frequently such white matter degeneration occurs because it has not been looked for in acute head injuries. In our series of 26 patients dying from head injuries six weeks or more after the accident, it was the significant pathological finding in about one-third of the cases.

It is impossible to say whether the nerve fibre damage is at any time reversible and what part, if any, it plays in the production of the signs of concussion, but the possibility that it may play a part should be borne in mind.

It should be remembered that such a diffuse white matter lesion may have to be looked for with care and that it is not enough to examine the brain-stem reticular formation or the cerebral cortex when investigating cases of severe disturbance of consciousness after head injury.

(Author's Abstr.)

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